

MIND

A QUARTERLY REVIEW

OF

PSYCHOLOGY AND PHILOSOPHY

I.—HR. VON WRIGHT ON THE LOGIC OF INDUCTION* (I.).

BY C. D. BROAD.

HR. VON WRIGHT is already known to those readers of *MIND* who are interested in Inductive Logic and Probability by his article on *Probability* in Vol. XLIX. The longer of the two essays now to be discussed is the thesis which he submitted successfully in 1941 for his doctor's degree at the University of Helsingfors; the shorter is an elaboration of Chapter IV of the thesis, contributed by him to a Scandinavian philosophical journal of which I do not know the name. The former is in English, the latter in Swedish. Together they constitute, so far as they go, the best treatment known to me of the problem of Induction. For this reason, and because it is most unlikely that either of them will be generally accessible in England before the end of the non-Japanese part of the present war, I propose to write an article round them rather than a review of them in the ordinary sense of the word.

I will begin by saying that the thesis is written in excellent English. It is not for me to comment on the Swedish of the short article; but I found it perfectly clear and easy to follow, and it is reasonable to suppose that an author who can express himself in a foreign language so well as Hr. von Wright has done would handle his native tongue with even greater skill.

* G. H. von Wright: (1) "The Logical Problem of Induction," *Acta Philosophica Fennica*, Fasc. 3, 1941. Pp. 257. (2) *Några Anmärkningar om nödvändiga och tillräckliga Betingelser*. Pp. 20.

The scope of the thesis is best indicated by the following summary of its contents. Chapter I is a brief introduction. Chapters II and III, entitled respectively *Induction and Synthetic Judgments a priori* and *Conventionalism and the Inductive Problem*, deal with what Hr. von Wright calls attempts to justify induction *a priori*. Chapter IV, entitled *Inductive Logic*, is concerned with attempts to justify induction *a posteriori* as leading to conclusions which are certain. It contains Hr. von Wright's treatment of necessary and sufficient conditions and the formal logic of methods of elimination such as Mill's. It must be taken along with the short article in Swedish on *Necessary and Sufficient Conditions* which carries the matter further. Chapters V, VI, VII, and VIII, entitled respectively, *Induction and Probability*, *Formal Analysis of Inductive Probability*, *Inductive Probability and the Justification of Induction*, and *Induction as a Self-correcting Process*, are concerned with attempts to justify induction *a posteriori* as leading to conclusions which are only probable. Chapter IX is a summary of the results reached in the course of the work. It is followed by 50 pages of *Notes*, and a *Bibliography* containing the names of 188 authors and 292 of their publications. The Notes and the Bibliography bear ample witness to the wide and deep foundation of knowledge on which Hr. von Wright has built his own conclusions, and they should be most useful to anyone who is working at the subject.

(I) PRELIMINARY CONSIDERATIONS.

(1) *Statistical Propositions*.—Let us call any proposition of the form ' $p\%$ of the instances of Q are instances of R ' a *Statistical Proposition*. Here p may have any value from 0 to 100, both inclusive. Now Hr. von Wright makes a very important point about the distinction between 100% and 0% statistical propositions, on the one hand, and Universal Affirmative and Universal Negative propositions, respectively, on the other. If the class of instances of Q is finite (e.g., the counters in a certain bag or the throws which have been made with a certain coin), the proposition ' 100% of Q 's are R ' is logically equivalent to ' $\text{All } Q\text{'s are } R$ ' and the proposition ' 0% of Q 's are R ' is logically equivalent to ' $\text{No } Q\text{'s are } R$ '. But, if the class of instances of Q is in principle unlimited (e.g., the series of possible throws with a coin), the meaning of ' $p\%$ of Q 's are R ' has to be carefully defined; and, when this is done, it is found that the equivalences break down and are replaced by one-sided entailments.

Hr. von Wright's definition of the statement 'the proportion of Q's which are R in an unlimited sequence of Q's is p ' may be summed up in the following two propositions. Let us first define the phrase 'an initial segment of a sequence' to mean any sub-sequence which consists of all the terms of the original sequence from the first to any given term, both inclusive. Then the two conditions which must be fulfilled if the proportion of Q's which are R in an unlimited sequence of Q's is to be p may be stated as follows:

(i) However small ε may be, every initial segment of the sequence of Q's is contained in a longer initial segment for which the proportion of Q's which are R does not differ from p by more than $\pm \varepsilon$; and

(ii) For any ratio other than p there is a quantity ε and an initial segment S_n such that for every initial segment which includes S_n the proportion of Q's which are R differs from this ratio by more than $\pm \varepsilon$.

Stated colloquially these two conditions come to this. As the sequence of Q's is extended further and further the proportion of them which are R reverts again and again to the immediate neighbourhood of p and does not revert again and again to the immediate neighbourhood of any other fraction.

Now it is important to notice the following facts. Either or both of these conditions might fail to be fulfilled. There might be no ratio to which the proportion of Q's which are R again and again reverts, or it might revert again and again to several different ratios. In either case there would be nothing that could be called *the* proportion of Q's which are R in the indefinitely extended sequence of Q's. Again, it is quite possible that the proportion of Q's which are R in an indefinitely extended sequence should be 100 % and yet that there should be Q's in the sequence which are not R. Indeed, whatever number we choose to mention, there might be more Q's than this in the sequence which are not R. Similarly, the proportion of Q's in the sequence which are R might be 0 %, and yet there might be more Q's in it which are R than any number that we choose to mention. Let us call statistical propositions in which the proportion is 100 % or 0 % 'Extreme Statistical Propositions'. Then the position is that, if the sequence is indefinitely extensible, universal affirmative or negative propositions entail extreme statistical propositions, but the converse does not hold. This is an exceedingly important point which has often been overlooked by writers on Induction, including (I am ashamed to say) myself.

It is worth remarking, as Hr. von Wright does on page 181 of the thesis, that conditions (i) and (ii) together entail the following proposition.

(iii) However small ε may be, there is an initial segment S_m such that for every initial segment which contains it the proportion of Q's which are R differs from p by not more than $\pm \varepsilon$.

Stated colloquially this means that, if conditions (i) and (ii) are fulfilled, there is a stage in the sequence after which the ratio of Q's which are R to Q's remains in the immediate neighbourhood of p . This can be proved by showing that the conjunction of (i) with the denial of (iii) entails the denial of (ii). This may be left as an exercise for the reader.

Before leaving this part of the subject we shall find it worth while to introduce a suitable notation to express the ideas outlined above. Let $f_n(R; Q)$ denote the proportion of Q's which are R in the first n of the sequence of Q's. Let $f(R; Q)$ denote the proportion of Q's which are R in the unending sequence of Q's. Let $x \doteq p \pm \varepsilon$ denote that x does not differ from p by more than ε ; and let $x \triangleq p \pm \varepsilon$ denote that x does differ from p by more than ε . Then what we have done is to define $f(R; Q)$ in terms of $f_n(R; Q)$; and to show that $f(R; Q) = 1$ is compatible with an indefinitely large number of Q's not being R, whilst $f(R; Q) = 0$ is compatible with an indefinitely large number of Q's being R. The symbolic expressions for propositions (i), (ii), and (iii) are as follows:

- $$\begin{aligned} & \{ \text{(i)} (\varepsilon, n) : (\exists N) . N > n \ \& \ f_N(R; Q) \doteq p \pm \varepsilon \\ & \text{(ii)} q \neq p . \supset_q : (\exists \varepsilon, n) . N > n \supset_N f_N(R; Q) \triangleq q \pm \varepsilon \\ & \text{(iii)} (\varepsilon) : (\exists n) . N > n \supset_N f_N(R; Q) \doteq p \pm \varepsilon. \end{aligned}$$

Hr. von Wright condenses conditions (i) and (ii) into a single formula. This is perfectly legitimate, but I think that it makes things clearer to express them separately. So far as I can see Hr. von Wright's formula contains two small errors of detail. In the first place, an implication-sign is written where the symbol '&' seems to be plainly required; and secondly he writes the symbol '=' where the symbol ' \doteq ' or some equivalent for it is needed.

(2) *Inductive Inferences.*—The premiss of an inductive argument is either (1.1) a *singular* proposition of the form 'This instance of Q is R', or (1.2) a *statistical* proposition of the form ' p % of the n instances of Q which have been observed are R'. This may be either (1.21) *extreme*, i.e., p may be 100 % or 0 %, or (1.22) *intermediate*, e.g., $p = 47$ %. The conclusion of an

inductive argument may be either (2.1) a *singular* proposition of the form 'The next instance of Q to be examined will be R'; or (2.2) a *statistical* proposition, which may be either (2.21) *extreme*, or (2.22) *intermediate*, i.e., may state that 100 % or 0 % or some intermediate percentage of the unending sequence of Q's will be R; or (2.3) a *universal* proposition (affirmative or negative), i.e., that all (or none) of the unending sequence of Q's will be R.

If a premiss of the form (1.1) is combined with a conclusion of the form (2.1) we have an inductive inference from *Singulars* to *Singulars*. If a premiss of the form ' p % of the n observed Q's are R' is combined with a conclusion of the form ' p % of all the Q's will be R', we have a *Statistical Generalisation*, no matter whether p be 0 or 100 or some intermediate percentage. If a premiss of the form (1.21) is combined with a conclusion of the form (2.3), i.e., if we infer a universal proposition about the unending sequence of Q's from an extreme statistical premiss about the n Q's which have been examined, the argument is a *Universal Generalisation*. As Hr. von Wright points out, there has been a tendency among writers on induction to confuse 0 % and 100 % statistical generalisations, on the one hand, with negative and affirmative universal generalisations, respectively, on the other.

The above are the most important types of Inductive Inference. Hr. von Wright subdivides Universal Generalisations into four kinds. His classification does not seem to be very systematic. In point of fact they can be divided, as follows, into four main species; and the third of these can be sub-divided into five sub-species and the fourth into four; so that in all there will be eleven ultimate sub-divisions. The classification proceeds as follows.

Let ρ and σ be two relations. Then (1) we have the simplest type, where the conclusion involves no relations but is of the form 'All Q's are R'. (2) Next we introduce one relation ρ . Then we have a generalisation involving just one quality and one relation, viz., 'Every pair of instances of Q stand in the relation ρ to each other'. (3) Next we have generalisations involving one relation and two qualities. Plainly there are five possibilities here. I will give in words two of them. (3.1) 'All instances of Q stand in the relation ρ to some instance of R'; and (3.4) 'Everything to which an instance of Q stands in the relation ρ is an instance of R'. (4) Lastly, we introduce a second relation σ . This gives rise to four possibilities. I will state the first and the fourth in words. (4.1) 'Everything which

has ρ to any instance of Q has σ to some instance of R '; and (4.4) 'To anything to which an instance of Q stands in the relation ρ an instance of R stands in the relation σ '.

These eleven possibilities can be symbolised as follows in Russell's and Whitehead's notation, if we write \hat{Q} for the class of instances of Q and \hat{R} for the class of instances of R .

$$(1) \hat{Q} \subset \hat{R}.$$

$$(2) x, y, \varepsilon \hat{Q} \supset x, y \rho(x, y).$$

$$(3.1) \hat{Q} \subset \rho \hat{R}. \quad (3.2) \hat{Q} \subset \tilde{\rho} \hat{R}. \quad (3.3) \rho \hat{Q} \subset \hat{R}. \quad (3.4) \tilde{\rho} \hat{Q} \subset \hat{R}.$$

$$(3.5) x \varepsilon \hat{Q} \& y \varepsilon \hat{R} \supset x, y \rho y.$$

$$(4.1) \rho \hat{Q} \subset \sigma \hat{R}. \quad (4.2) \tilde{\rho} \hat{Q} \subset \sigma \hat{R}. \quad (4.3) \rho \hat{Q} \subset \tilde{\sigma} \hat{R}.$$

$$(4.4) \tilde{\rho} \hat{Q} \subset \tilde{\sigma} \hat{R}.$$

The four cases which Hr. von Wright distinguishes are our (1), (2), (3.1) and (3.4). He calls (3.1) 'Existential Hypotheses'. (3.2) is the heading under which Uniformities of Sequence fall. For any such proposition is of the form 'Any instance of Q is immediately followed in time and adjoined in space by an instance of R '.

The logical problem of justifying the transition from the 100 % statistical premiss based on n observed instances to the universal conclusion is precisely the same for all the eleven cases, so we can confine our attention henceforth to the simplest of them.

(II) ATTEMPTS TO JUSTIFY INDUCTION *A PRIORI*.

When Hr. von Wright calls an attempt to justify the generalisation 'All instances of Q will be R ' *a priori* what he means is this. The fact that n instances of Q have been observed and that 100 % of them were R is to be no part of the *premisses* from which 'All Q 's will be R ' has been *inferred*, whether with certainty or with probability. The observations may have been an indispensable pre-requisite psychologically, *e.g.*, it may be that without them no-one would have had an idea of the characteristics Q and R or would have entertained the notion of their being conjoined. The question is whether, when all these psychological pre-conditions have been fulfilled, a person can know or rationally conjecture that the presence of Q necessarily carries with it that of R . If this were possible in any case, he could then infer that every instance of Q would

be an instance of R. There are two alternatives to be considered, *viz.*, the claim that causal laws are necessary and synthetic and the claim that they are analytic.

(1) *Causal Laws as Synthetic Necessary Propositions.*—It seems to me that there are two ways of attacking such attempts to justify inductive generalisations. One is analytical, the other is epistemological. The analytical way is to analyse the notions of 'necessary' and 'synthetic', as applied to facts or propositions, and to try to show that the notion of a necessary synthetic fact or proposition is meaningless. This is the more radical method; for, if there can be no such facts or propositions, it follows at once that no-one can know any of them. The epistemological way is to leave the possibility of synthetic necessary facts or propositions an open question; but to argue that, if there are any, we are never in a position to know them. This contention might itself take a more or less radical form. It might be argued (i) that, with regard to all synthetic propositions about nature, we *can* see that they *are not* necessary; or (ii) that, with regard to no synthetic proposition about nature, can we see that it is necessary. The milder form of the epistemological contention would be enough to wreck attempts to justify inductive generalisations *a priori*. Hr. von Wright takes the radical analytic path; and obviously if it is open to traffic it is the shortest and quickest.

Hr. von Wright contends that the proposition 'There can be no necessary synthetic propositions' is itself necessary and analytic. The essence of his argument is as follows. Consider the statement 'All instances of Q are necessarily instances of R'. This means that the proposition 'This is an instance of R' *follows from* 'This is an instance of Q'; and this means that the disjunctive proposition 'This is either not-Q or R' is a *tautology*. On the other hand, to say that 'All instances of Q are instances of R' is synthetic means that the two characteristics Q and R are *not* such that to be R follows logically from being Q. Therefore the statement 'There can be no necessary synthetic propositions' is itself a tautology.

Hr. von Wright realises that no-one who believes that there are or may be necessary synthetic propositions is likely to be much moved by this line of argument. The immediate reaction of such a person will be to say: 'I distinguish between purely formal or logical necessity and another kind of necessity. Even if purely formal necessity can be defined in terms of following logically, and even if the latter can be defined in terms of

tautology, I am concerned, not with it, but with what may be called *non-formal necessity*.

His answer to such a contention might be put as follows: 'Your non-formal necessity will be irrelevant for the present purpose unless you can infer from it that every instance of Q will be R. Now suppose (what you cannot deny to be intelligible and possible) that someone were to allege that an instance of Q which was not R had been found. What are you to say about it? Either (i) you may admit that it is a genuine counter-instance, or (ii) you may deny this. On the first alternative you will have to admit that you were mistaken in thinking that the presence of R follows necessarily but non-formally from that of Q. On the second alternative you will have to say either (a) that this was not really an instance of Q, though it seemed to be one, or (b) that this was really an instance of R, though it seemed not to be one. Suppose that renewed and more careful observation of the alleged counter-instance shows that it answers all the tests hitherto accepted for the presence of Q and that it fails to answer some of the tests hitherto accepted for the presence of R. At this stage you can save the situation only by refusing to call anything an instance of Q unless it is manifestly an instance of R also; or by insisting on calling a thing an instance of R, in spite of all appearances to the contrary, if it is an instance of Q. But in that case you have made being R part of the definition of being Q, and you have saved your non-formal synthetic necessity only at the price of turning it into a formal analytic necessity.'

Hr. von Wright thinks that the above represents the kernel of Hume's argument about Causation, when stripped of epistemological and psychological features which were non-essential. He next considers Kant's attempt to answer Hume.

According to Hr. von Wright Kant did prove something important in the Analogies of Experience and he did thereby fill a serious gap in Hume's philosophy, but what he proved was quite irrelevant to the question of justifying inductive generalisations. Kant showed that all transition from sense-data propositions to physical-object propositions depends upon certain invariant relations among sense-data. *If* intersubjective intercourse is to remain possible, *if* it is to remain possible to draw a distinction between the temporal order in which each of us happens to get his sensations and an objective temporal order of physical events, certain very general kinds of regularity, which have held in the past, must continue to hold in the future among our sensations. But we have no means of

knowing that the antecedents of these hypotheticals will always be fulfilled.

Moreover, as Kant came to recognise when he wrote the *Critique of Judgment*, even if he had established the Law of Universal Causation as an absolutely, and not merely a hypothetically, necessary proposition, he would not have taken us a step towards justifying any particular inductive generalisation.

Hr. von Wright next gives an account of the views of Fries and his school. These philosophers pointed out that no answer can be given to Hume along Kantian lines. They held that certain synthetic propositions are, and can be seen by inspection to be, necessary, and that it is a mistake to ask for a proof of them. This type of theory was most fully stated by Apelt, who held that the fundamental laws of nature are necessary and synthetic but hypothetical propositions which can be known by mere reflection on the characteristics involved in them. They are of the form 'If anything were Q it would necessarily be R'. The only function of experience in natural science is to assure us that there are in fact instances of Q. But, when Apelt faces the possibility of apparent exceptions to these synthetic and necessary laws of nature, his solution is indistinguishable from the doctrine that the laws are true by the definitions of their terms and are therefore analytic.

Under the head of *Some other Theories of Causation* Hr. von Wright gives a brief discussion of Whitehead's theory of Causal Perception, Meyerson's account of scientific explanation, and Bradley's and Bosanquet's theory of Concrete Universals, considered as contributions towards an *a priori* justification of inductive generalisations. His discussion may be summarised as follows.

Whitehead gives a much more satisfactory psychological account than Hume of the conditions under which we do in fact make anticipations and generalisations when confronted with concrete situations. But this provides no *a priori* guarantee that such generalisations and anticipations will hold without exception in the future.

Hr. von Wright interprets Meyerson's theory that scientific explanation consists in showing the 'identity' of cause and effect to mean that it consists in showing that the consequent of a causal proposition is logically entailed by its antecedent. He admits that this is very often true (it is in fact the element of truth in the Conventionalist Theory, which has yet to be discussed); but, where it holds, causal laws give no justification for making predictions. I should doubt whether the above

interpretation of Meyerson's theory is altogether adequate. I think that he was more concerned with another sense in which cause and effect may be said to be 'only psychologically different', viz., where there is quantitative identity in some important respect (e.g., conservation of mass or of energy) together with qualitative dissimilarity. This kind of identity between cause and effect does not make causal laws analytical, though it certainly does nothing towards providing an *a priori* justification for inductive inference.

The interpretation which is put on Bosanquet's theory of Concrete Universals in relation to inductive inference is as follows. A concrete universal is identified with a Natural Kind, in Mill's sense of the word. The theory assumes (i) that all instances of a given Natural Kind K have the same causal properties, and (ii) that the presence or absence of a few easily recognisable qualities in an individual is a conclusive test of whether it does or does not belong to a certain Natural Kind. We may admit the importance of the facts which are expressed by the doctrine of Natural Kinds, but we are faced with the usual dilemma if an instance should be met which answers all the tests for belonging to the Natural Kind K and yet does not have the effects which members of that Kind have hitherto been found to have. (That this is not a purely imaginary case is well illustrated by the discovery of isotopes.) In presence of such cases we have either to give up the alleged causal law or to save it by making it an analytical proposition.

(2) *Causal Laws as Analytical Propositions.*—Hr. von Wright introduces the subject of the part played by conventions in science by discussing two examples, the melting-point of phosphorus and the impact of billiard balls.

We have become familiar with instances of a kind of matter which have had certain fairly easily recognisable characteristics, X, Y, and Z, severally common and collectively peculiar to them. We have given the name 'Phosphorus' to matter of this kind, without necessarily committing ourselves to *defining* 'Phosphorus' as matter which has the properties X, Y, and Z, or indeed in any other way. We find that all the instances of Phosphorus on which we have tried the experiment melt at 44°C. , and we generalise this into the law 'Phosphorus melts at 44°C. '. Then we meet with a bit of matter which has the properties X, Y, and Z but does not melt at 44°C.

If our generalisation means 'Everything that has the qualities X, Y, and Z melts at 44°C. ', it has been refuted. But we may

admit the counter-instance and yet save the law 'Phosphorus melts at 44° C.' in at least two ways. (i) We may set about defining 'Phosphorus', and we may decide to make melting at 44° C. part of the definition. (ii) Another course, which we are more likely to follow, is to say that 'Phosphorus' is a substance of a certain molecular and atomic structure; and that melting at 44° C. is an infallible sign of this structure, whilst the conjunction of qualities X, Y, and Z, though in general a trustworthy indication, is not an infallible sign of it. In either case the generalisation has been saved by being made analytical. (I can well remember from the days when I did organic chemistry the following situation. We were told that so-and-so melts at n° C. We tried an alleged sample of so-and-so and found that it did not melt exactly at n° C. This was explained by saying that these samples were not 'chemically pure' so-and-so. And, finally, when one asked for a criterion for determining when a sample of so-and-so is chemically pure, one was told that the most reliable and convenient test was to see whether it melted at n° C.)

Consider now the law that a billiard ball starts to move when hit by another ball. So soon as we meet with counter-instances we find that all kinds of qualifying conditions, some positive and some negative, have to be inserted. The ball must not be stuck to the table, the moving body which strikes it must not be too light, and so on. Most of these conditions are generally fulfilled or are so obvious that we do not need to mention them. Now we are liable to say that, when all relevant circumstances have been explicitly introduced into the statement of a law, it will hold without exception. But this can be guaranteed only in one way, *viz.*, by adopting the convention that, when all the known relevant conditions are fulfilled and yet the consequent does not follow, we shall say that there must be some unknown relevant condition which is unfulfilled in the present case.

Generally the considerations which have been mentioned in the example of the melting-point of Phosphorus and those which have been mentioned in the example of impact are both involved together. *E.g.*, we talk of the melting-point of a substance; but we very soon find that the melting-point of any substance varies with the pressure, that Phosphorus exists in different allotropic forms with different melting-points under the same pressure, and so on. Take, *e.g.*, the 'law' that water boils at 100° C. under normal atmospheric pressure. This might be regarded as part of the definition of 'pure water', or again as the definition of ' 100° C.'

There is, then, no doubt that generalisations which begin by being synthetic and contingent, very often end by becoming analytic and necessary. As the transition is gradual, one is very liable to combine in a muddled way the synthetic character of their earlier phases with the necessary character of their later phases, and so to think of them as being both synthetic and necessary throughout their history.

Some philosophers have thought that the fact that scientific laws tend to become analytical propositions disposes of the problem of justifying inductive inferences. Hr. von Wright has no difficulty in showing that they are mistaken. Suppose that the proposition 'Phosphorus melts at 44° C.' has become analytic. Associated with it is the synthetic proposition 'Anything that answers to all the tests for Phosphorus other than that of melting at 44° C. will also melt at 44° C.' Undoubtedly we are strongly inclined to believe this proposition, and to act on our belief. But the mere fact that we should not call a substance 'Phosphorus' if it failed to melt at 44° C. is no ground for this belief. What causes and what seems to justify belief in the synthetic proposition is, not the analytic proposition, but the mass of empirical facts which have given rise to the convention which has made 'Phosphorus melts at 44° C.' analytic. So we are back, where we started, at the problem of justifying a synthetic universal generalisation on the basis of a 100 % statistical proposition about a limited class of observed instances.

(III) ATTEMPTS TO JUSTIFY INDUCTION A *POSTERIORI*.

(A) DEMONSTRATIVE.

An attempt to justify an inductive generalisation is a *posteriori* if it uses as a premiss the fact that such and such instances have been observed and have been found to have such and such characteristics. Such an argument may claim to be either demonstrative or only problematic. In the former case the observations, either alone or in conjunction with certain other premisses, are alleged to entail the generalisation. In the latter case they are alleged only to make the generalisation *highly probable*. At present we are concerned only with demonstrative arguments. Hr. von Wright distinguishes two kinds of attempt at demonstrative justification, *viz.*, the theory of induction as an Inverse Process of Deduction and the theory of induction as an Eliminative Process.

(1) *Induction as the Inverse of Deduction.*—This method is associated with Jevons and particularly with Whewell, but both Galileo and Leibniz had already made statements about induction which seem to imply this theory of it.

In essence the account which it gives of induction is that we first brood over a mass of observed data and try to conjecture a generalisation which will fit them all, and then we see whether the data can be deduced from the conjectured generalisation. If they can, we say that the generalisation has been 'verified'. The classical instance of this is the discovery and verification of the law that the planets move in ellipses about the sun as focus, which was the subject of so much controversy between Mill and Whewell.

The main points which Hr. von Wright makes are these.

(i) Opponents of Whewell were inclined to confine their attention to very simple cases where there is no difficulty in guessing a suitable generalisation and no doubt that the generalisation proposed fits all the data. They overlooked the fact that in advanced sciences it may be very difficult to think of any simple generalisation that covers the data, and that when one has done so elaborate deduction may be needed to show that it fits them.

(ii) On the other hand, Whewell's theory gives no justification for believing that the generalisation which fits all the data extends beyond them, *e.g.*, that the unobserved intermediate positions of the planet fell on the curve which fits the observed positions or that its future positions will fall on the same curve as its past ones.

In fact all that Whewell's method will prove is that a certain generalisation is *one* of those which is *consistent with* all the *observed instances*. In order to justify inductive inference demonstratively we should have to prove that there is one and only one generalisation consistent with the data and that this will apply also to instances not included among the data.

(2) *Induction as an Eliminative Process.*—Hr. von Wright treats eliminative induction in terms of the notions of Sufficient and of Necessary Conditions. I am quite sure that he is correct in this, and I welcome his elaborate discussion all the more because I adopted the same line of approach in the first of my two articles on *The Principles of Demonstrative Induction* in Vol. XXXIX of MIND. Hr. von Wright's treatment has one great advantage over mine. By recognising the possibility of *disjunctive* necessary conditions, beside that of *conjunctive* sufficient conditions, he introduces a symmetry and completeness

which were lacking in my treatment. I propose now to give, in my own way and in my own notation, an account of necessary and sufficient conditions based on what Hr. von Wright has put forward in the Thesis and with greater elaboration in the Article.

I will first make some remarks about notation. I shall use small letters, such as p , q , and r , to denote characteristics which are *simple*, i.e., involve neither negation, conjunction, nor disjunction. I shall use large letters, such as P , Q , and R , to denote characteristics which *may* be simple but may also be synthesised out of simple characteristics by single or repeated or combined applications of negation, conjunction, and disjunction. Thus, e.g., P would cover such cases as p , \bar{p} , $p \vee q$, $p \& \bar{q}$, $\vee . r$, and so on. I shall denote the proposition ' x is P ' by $P(x)$, the proposition ' x is P and Q ' by $P \& Q(x)$, and the proposition ' x is P or Q ' by $P \vee Q(x)$. I shall use Q to denote a characteristic of which we are seeking either the sufficient or the necessary conditions. I shall denote its possible sufficient conditions by P_1 , P_2 , etc., and its possible necessary conditions by R_1 , R_2 , etc.

The statement that P is a sufficient condition of Q means that every instance of P is an instance of Q . It may be symbolised by $P \sigma Q$; so we have

$$P \sigma Q = P(x) \supset_x Q(x) \text{ Df.}$$

The statement that R is a necessary condition of Q means that every instance of Q is an instance of R . It may be symbolised by $R \nu Q$; so we have

$$R \nu Q = Q(x) \supset_x R(x) \text{ Df.}$$

The following propositions follow immediately from these definitions.

- (i) $R \nu Q \equiv Q \sigma R$. (ii) $P \sigma Q \equiv \bar{Q} \sigma \bar{P}$. (iii) $R \nu Q \equiv \bar{Q} \nu \bar{R}$.
 (iv) A sufficient condition of a sufficient condition of Q is a sufficient condition of Q . (v) A necessary condition of a necessary condition of Q is a necessary condition of Q . (vi) If P is a sufficient condition of Q it is a sufficient condition of every necessary condition of Q . (vii) If R is a necessary condition of Q every sufficient condition of Q is a sufficient condition of R .

The next point to notice is a certain lack of symmetry between necessary and sufficient conditions. (i) It is plain from the definition of $R \nu Q$ that, if Q has any necessary conditions, they must *all* be present in *every* instance of Q . If we contrapose this, we get the equivalent negative proposition 'No characteristic which is absent from *any* instance of Q can be a *necessary*

condition of Q . An immediate consequence of this is that, if Q has several necessary conditions, they must all be logically and causally compatible with each other. (ii) On the other hand, it is plain from the definition of $P \circ Q$ that, even if Q has some sufficient condition in every instance in which it occurs, there is no need for all its sufficient conditions to be present in even a single instance of it. In some instances the sufficient condition P_1 might be present, in others this might be absent and another sufficient condition P_2 might be present, and so on. It is plain, then, that Q might have a number of sufficient conditions which are logically or causally incompatible with each other. (iii) What corresponds, in the case of sufficient conditions, to proposition (i) about necessary conditions is the following. If Q has any sufficient conditions, they must all be *absent* from *any* instance from which Q is *absent*. If we contrapose this, we get the equivalent negative proposition 'No characteristic which is present in any instance from which Q is absent can be a sufficient condition of Q '. It is on these two principles that eliminative induction rests.

We pass now to the distinction between simple and composite conditions. It follows at once from our definitions that *disjunctively* composite *sufficient* conditions and *conjunctively* composite *necessary* conditions are of no particular interest. For it is immediately obvious that

$$(p_1 \vee p_2) \circ Q \equiv : p_1 \circ Q \cdot \& \cdot p_2 \circ Q$$

and

$$(r_1 \& r_2) \nu Q \equiv : r_1 \nu Q \cdot \& \cdot r_2 \nu Q.$$

On the other hand, *conjunctively* composite *sufficient* conditions and *disjunctively* composite *necessary* conditions are interesting and important. It may be that p_1 is not a sufficient condition of Q and that p_2 is not a sufficient condition of Q , but that the conjunction $p_1 \& p_2$ is a sufficient condition of Q ; i.e., there may be instances of p_1 which are not Q and instances of p_2 which are not Q , but no instances of $p_1 \& p_2$ which are not Q . Again, it may be that r_1 is not a necessary condition of Q and that r_2 is not a necessary condition of Q , but that the disjunction $r_1 \vee r_2$ is a necessary condition of Q ; i.e., there may be instances of Q which are not r_1 and instances of Q which are not r_2 , but no instances of Q which are not either r_1 or r_2 .

This leads to the notion of what I called a 'Smallest Sufficient Condition' in my article on *Demonstrative Induction*. I see now that this must be supplemented by the parallel notion of what I will call a 'Smallest Necessary Condition'. The definitions

of these notions are as follows. P is a smallest sufficient condition of Q if it is a sufficient condition of Q, and either (i) it is a simple characteristic p , or (ii) it is a conjunctive characteristic $p_1 \& p_2 \& \dots p_n$ such that if any of the conjuncts be omitted what remains is not a sufficient condition of Q.

R is a smallest necessary condition of Q if it is a necessary condition of Q, and either (i) it is a simple characteristic r , or (ii) it is a disjunctive characteristic $r_1 \vee r_2 \vee \dots r_n$ such that if any of the alternants be omitted what remains is not a necessary condition of Q.

Even if Q has a sufficient condition in every instance in which it occurs, it is possible that none of its sufficient conditions is simple. And, if Q has necessary conditions, it is possible that none of them are simple. I propose to call any simple characteristic or conjunction of such characteristics which is part of any smallest sufficient condition of Q a *Contributory Condition* of Q. Thus every smallest sufficient condition of Q is either simple or is a conjunction of a number of simple contributory conditions. I propose to call any simple characteristic or disjunction of such characteristics which is part of any smallest necessary condition of Q a *Substitutable Requirement* of Q. Thus every smallest necessary condition of Q is either simple or is a disjunction of a number of simple substitutable requirements.

It is evident from the definition of a sufficient condition that, if P is a sufficient condition of Q, then the conjunction of P with *any* other characteristic is also a sufficient condition of Q. Similarly, if R is a necessary condition of Q, the disjunction of R with *any* other characteristic is also a necessary condition of Q. The notions of smallest sufficient and smallest necessary conditions are important in cutting out the trivialities which would otherwise arise from these facts.

It would be possible to arrange simple contributory conditions in a kind of hierarchy of what I will call 'Dispensability' in the following way. (i) Suppose that Q has one and only one smallest sufficient condition. Then we can say that all its simple contributory conditions are 'equally indispensable'. (ii) Suppose that Q has several smallest sufficient conditions. It may be that some of its simple contributory conditions are present in all of these, that some are present in all but one of them, that some are present in all but two of them, and so on. Then we could say that those of the first kind are 'indispensable', that those of the second kind have 'dispensability of the first degree', that those of the third kind have 'dispensability of the second degree', and so on.

Lastly, it is worth remarking that several of Q's smallest sufficient conditions might be present together in the same instance of Q. In that case I should say that Q was 'over-determined'. *E.g.*, a person may believe that a certain decision would be right and also that it would give pleasure to himself. Either of these beliefs, in conjunction with his conative and emotional dispositions, might suffice, in the absence of the other, to determine this decision. If so, the decision is over-determined.

We come now to a very important point which Hr. von Wright makes, and which I had also made in my article on *Demonstrative Induction*. There is nothing in the definition of a sufficient condition, to guarantee either (i) that every characteristic has a sufficient condition, or (ii) that, even if Q has one or more smallest sufficient conditions, there may not be instances in which Q occurs without any of these sufficient conditions being present. Similarly, there is nothing in the definition of a necessary condition to guarantee that every characteristic has a necessary condition. An immediate consequence of this is that it is logically possible that P should be an *indispensable contributory* condition of Q without being a *necessary* condition of Q. For to say that it is an indispensable contributory condition of Q is to say that it is a conjunct in all the smallest sufficient conditions of Q, whilst to say that it is a necessary condition of Q is to say that it is present in every instance of Q. Now, if there can be instances of Q in which none of its smallest sufficient conditions are present, it is plain that there may be instances of Q in which none of its indispensable contributory conditions are present.

The minimum assumption that will avoid these consequences is the following. Let us assume that, whatever characteristic Q may be, every instance in which it occurs is characterised by some sufficient condition of it. This is what I called the *Postulate of Smallest Sufficient Conditions* and it is one form in which the Law of Causation might be stated. It follows at once from this postulate that the disjunction of all Q's smallest sufficient conditions is a necessary condition of Q. And from this it follows immediately that any indispensable contributory condition which Q may have is a necessary condition of Q.

It is easy to show, as Hr. von Wright does, that it follows from the same postulate that the conjunction of all Q's necessary conditions is a sufficient condition of Q. For, as we have seen, it follows that *one* of Q's necessary conditions is the disjunction of all its smallest sufficient conditions. But this disjunction is also a sufficient condition of Q. Therefore the conjunction of

it with anything else (and therefore with the rest of Q's necessary conditions) is a sufficient condition of Q.

I very much doubt whether this proposition is what people have in mind when it seems self-evident to them that a conjunction of all Q's necessary conditions must be a sufficient condition of Q. I suspect that when people talk of 'necessary conditions' they are often thinking of contributory conditions. Every contributory condition of Q is necessary, not indeed to Q itself, but to at least one of Q's smallest sufficient conditions. And it is an analytical proposition that a conjunction of all Q's contributory conditions would constitute a sufficient condition of Q; for between them they would constitute *all* Q's smallest sufficient conditions, and would thus in general over-determine Q.

This is as much as I need say about the formal logic of sufficient and of necessary conditions. It remains to consider the application of it to eliminative induction.

As Hr. von Wright points out, it is plain that there are two and only two fundamental 'Methods' of eliminative induction. One is concerned with eliminating possible sufficient conditions, and cannot be used directly for dealing with possible necessary conditions; the other is concerned with eliminating possible necessary conditions, and cannot be used directly to deal with possible sufficient conditions.

Suppose we want to find the *necessary* conditions of Q. We rely on the principle that no characteristic which, absent in any instance of Q, can be a necessary condition of Q. We therefore take a number of instances of Q which agree in as few respects as possible except the presence of Q. We find what is common to all of them other than Q itself. Then this common part contains all the necessary conditions of Q. It may, of course, contain characteristics which are not necessary conditions of Q; and further and more variegated instances of Q might enable us to eliminate some of these. It is evident that this is in essence the *Method of Agreement*.

Hr. von Wright does not tell us in detail how to perform the process of finding all the possible necessary conditions which are consistent with a given set of instances of Q, so I will take an example to illustrate the Method of Agreement. The rule may be stated as follows. 'Take the disjunction of all the conjunctions of characteristics other than Q, in each of the instances. Express this as a conjunction of terms in which each conjunct is either (a) simple, or (b) a disjunction of simple terms. Then each conjunct is a possible necessary condition of Q.' Now for an example.

Suppose that we have the three instances $Q \& r_1 \& r_2 \& r_3$ (a), $Q \& r_1 \& r_2 \& \bar{r}_3$ (b), and $Q \& r_1 \& \bar{r}_2 \& \bar{r}_3$ (c) to begin with. We take the disjunction

$$r_1 \& r_2 \& r_3 \cdot \vee \cdot r_1 \& r_2 \& \bar{r}_3 \cdot \vee \cdot r_1 \& \bar{r}_2 \& \bar{r}_3.$$

It is very easy to show that this boils down to $r_1 \& r_2 \vee \bar{r}_3$. So at this stage the possible necessary conditions of Q are r_1 and $r_2 \vee \bar{r}_3$, i.e., one simple condition and one composite disjunctive condition. Suppose now that a further instance $Q \& \bar{r}_1 \& \bar{r}_2 \& \bar{r}_3$ (d) is observed. We must now take the disjunction of $\bar{r}_1 \& \bar{r}_2 \& \bar{r}_3$ with what was left standing by the first three instances, viz., $r_1 \& r_2 \vee \bar{r}_3$. This works out to $r_1 \vee \bar{r}_2 \cdot \& \cdot r_1 \vee \bar{r}_3 \cdot \& \cdot r_2 \vee \bar{r}_3$. It is interesting to note that the additional instance has not reduced the *number* of possible necessary conditions of Q . It has in fact increased it from two to three. But it has reduced the *strength* of the conditions. For the first three instances left open the possibility that the simple characteristic r_1 might be a necessary condition of Q . The addition of the fourth instance has eliminated this possibility and shown that Q has no simple necessary condition, but at most disjunctive ones.

Suppose next that we want to find the *sufficient* conditions of Q . We rely on the principle that no characteristic which is present in any instance from which Q is absent can be a sufficient condition of Q . We also use the postulate that in every instance of Q there is a sufficient condition of it. In this case we take (i) an instance in which Q is present, e.g., $p_1 \& p_2 \& p_3 \& Q$ (a). (ii) A number of other instances which between them resemble the first as much as possible except in the fact that Q is absent in all of them. E.g., they might be $p_1 \& p_2 \& \bar{p}_3 \& \bar{Q}$ (b) and $\bar{p}_1 \& p_2 \& p_3 \& \bar{Q}$ (c). The argument would run as follows. In virtue of our postulate we know that the first instance must contain a smallest sufficient condition of Q . But this might be either $p_1 \& p_2 \& p_3$ or $p_1 \& p_2$ or $p_2 \& p_3$ or $p_3 \& p_1$ or p_1 or p_2 or p_3 . The first counter-instance eliminates the possibility that it is $p_1 \& p_2$ (and therefore also the possibilities that it is p_1 and that it is p_2). The second counter-instance eliminates the possibility that it is $p_2 \& p_3$ (and therefore also the possibilities that it is p_2 and that it is p_3). So at this stage the possibilities which remain are that the sufficient condition of Q in the first instance was either $p_1 \& p_3$ or $p_1 \& p_2 \& p_3$. Suppose now that another counter-instance $p_1 \& \bar{p}_2 \& p_3 \& \bar{Q}$ (d) were found. This would eliminate the possibility that $p_1 \& p_3$ is a sufficient condition of Q . We should be left with the conclusion that

nothing less than p_1 & p_2 & p_3 was sufficient to produce Q in the first of our instances, and therefore that p_1 , p_2 , and p_3 were all indispensable contributory conditions.

It is evident that the reasoning just described is in essence the *Method of Difference*. The following important difference between the ranges of the two Methods should be noted. Since *all* the necessary conditions of Q must be present in *every* instance of Q , a single instance gives us the field within which all possible necessary conditions of Q are contained. The further instances used by the Method of Difference simply serve to reduce this field. But we have no guarantee that all the smallest sufficient conditions of Q are present in any one instance of it. So the positive instance in the Method of Difference gives us only the field within which the smallest sufficient condition of Q in *that instance* must lie. The counter-instances may between them reduce that field to a single possibility, as they did in the example given above; but even so they leave open the possibility that Q may have many other smallest sufficient conditions in other instances of its occurrence.

Hr. von Wright points out the following important limitation of eliminative methods, whether applied to finding sufficient or necessary conditions. Either the process of elimination leaves several alternative possible necessary or sufficient conditions, as the case may be, still standing; or, if not, the one alternative left is, in the case of necessary conditions, the disjunction of *all* the simple characteristics in the various instances of Q , and, in the case of sufficient conditions, the conjunction of *all* the simple characteristics in the single instance of Q . Thus one always knows beforehand what the end of the process of elimination must be if it is to succeed in eliminating all alternatives but one. The reason for this is plain. Suppose, e.g., that the positive instance in the Method of Difference is p_1 & p_2 & p_3 & Q (a). Then evidently the whole conjunction p_1 & p_2 & p_3 is *one* candidate for the office of smallest sufficient condition of Q . Now all that the Method of Difference can do is to eliminate the claims of selections from it, such as p_1 & p_2 or p_3 , to be sufficient conditions. Therefore so long as any other candidate remains standing the most complex conjunction remains as a possible candidate. The argument is precisely similar in the case of necessary conditions and the Method of Agreement, with 'disjunction' substituted for 'conjunction'.

We have seen that it follows from the definition of ' P is a sufficient condition of Q ' that this is equivalent to ' \bar{P} is a necessary condition of \bar{Q} '. This fact enables us to use the

Method of *Agreement* indirectly for finding the *sufficient* conditions of Q . For this purpose we should have to take a number of instances which agree in the *absence* of Q but in other respects differ among themselves as much as possible. Any characteristic other than \bar{Q} which is common to all of them is a possible *necessary* condition of Q . Therefore the *negation* of any such characteristic is a possible *sufficient* condition of Q .

The following point is worth noting about this indirect application of the Method of Agreement. All the necessary conditions of \bar{Q} must be present in any instance of \bar{Q} . Therefore the negations of these will include *all* the sufficient conditions of Q . Thus this method gives us a field which includes all the sufficient conditions of Q , whilst the direct application of the Method of Difference for finding sufficient conditions is concerned only with the smallest sufficient condition of Q in the particular instance of Q under investigation.

In a similar way the Method of *Difference* may be used indirectly for finding the smallest necessary conditions of Q . This depends on the fact that, if Q is a necessary condition of R , then \bar{R} is a sufficient condition of \bar{Q} , and conversely. For this purpose we should have to take (i) an instance in which Q was *absent*, and (ii) a number of instances which between them resemble the first as much as possible except that Q is *present* in all of them. Let us suppose that the Postulate of Smallest Sufficient Conditions applies to negative characteristics, like \bar{Q} , as well as to positive characteristics like Q . Then the conjunction of characteristics other than \bar{Q} in the first instance must either be or contain a smallest sufficient condition of \bar{Q} . Any factor or combination of factors common to this and to one or more of the instances in which Q is present can be rejected from the class of possible sufficient conditions of \bar{Q} . What remain are possible sufficient conditions of \bar{Q} . Therefore the negations of these are possible necessary conditions of Q .

This completes the account of the formal logic of methods of elimination.

The question that remains is this. Is it possible to infer with certainty from data such as we have been considering, by means of Methods of Elimination, general propositions about the necessary or the sufficient conditions of a given characteristic Q ?

Hr. von Wright points out that this question may be divided into three, which are often not clearly distinguished, but which form a hierarchy. They may be formulated as follows. (i) Is it ever possible to show that, *if* Q has necessary or has sufficient

conditions, then the only hypothesis about these conditions which is consistent with our empirical data is so-and-so? (ii) Is it ever possible to show that, if certain general propositions about nature be granted in addition to the empirical data, then it would follow from the principles and the data together that the necessary or the sufficient conditions of *Q* are so-and-so? (iii) If so, can we know that these principles are true and therefore infer with certainty that the necessary or the sufficient conditions of *Q* are so-and-so?

The answer to the first question is Yes. But our enthusiasm over this answer is damped when we remember that we can be certain beforehand that, if the Method of Elimination does leave only one hypothesis standing, this will necessarily be the most complex one that is consistent with the data.

The answer to the second question is as follows. (a) It is quite evident that some general principle about nature must be added to the data if the Eliminative Method is to lead to any positive categorical conclusion. For the Method applied to the data alone leads directly only to negative results, *viz.*, that such and such hypotheses about the conditions of *Q* must be rejected as inconsistent with the data. Even if in this way we can eliminate all the alternative hypotheses but one, we have no right to accept the one survivor unless we are granted the premiss that *Q* has necessary conditions and that it has sufficient conditions not only in this instance but in every instance. As we have seen, the latter premiss carries the former with it. So what I have called the 'Postulate of Smallest Sufficient Conditions' and what Hr. von Wright calls the 'Deterministic Assumption' is certainly needed; but is it enough?

(b) It is certain that something else is needed too. In our examples of the Methods we have made it appear as if the instances under consideration have, and are known to have, only a small number of characteristics, all of which have been distinguished, recognised, and labelled with *p*'s or *r*'s. When we remember that the characteristics may include, beside pure qualities, relational properties both non-dispositional and dispositional, it is plain that this appearance is misleading. So, although it might be possible for an angel with a microscopic and a telescopic eye to fulfil the conditions for answering question (ii) in the affirmative without any other postulate but the Deterministic Assumption, it is certain that beings like ourselves are not in a position to do so unless some further postulate is granted. It is plain that the postulate needed is one that will place some limitation on the number of characteristics which

need to be considered in reference to the question: 'What are the necessary or the sufficient conditions of Q?'

Hr. von Wright points out that it would not be enough to know that the number of independent characteristics is finite, i.e., that there is some number or other which it does not exceed. The smallest assumption on these lines which would be of any use is that the number of independent characteristics does not exceed a certain assigned number, e.g., 1000. An alternative postulate would be that certain classes or characteristics can be ruled out as irrelevant to a given characteristic Q, and that we can have exhaustive knowledge of all the other characteristics of each instance of Q under consideration. In general what is wanted is some principle in virtue of which it is possible to *know* when we have exhaustive knowledge of all the characteristics of our instances which are relevant to Q. Hr. von Wright calls such a postulate, no matter what particular form it may take, the postulate of 'Completely Known Instances'.

So the third question depends for its answer on the answer to the question whether we know or have rational grounds for believing the Deterministic Assumption and some form of the postulate of Completely Known Instances.

Now there are two alternatives to be considered, viz., (i) that these postulates are *a priori* propositions, or (ii) that they are themselves empirical generalisations. I am not sure that I understand Hr. von Wright's argument about the consequences of supposing that the postulates are *a priori* propositions. I propose therefore to substitute for it the following argument, which appears to lead to much the same conclusions and may really be the same as his.

Suppose that A, B, and C are any three propositions such that A & B entails C. (E.g., A might be 'All men are mortal', B might be 'Socrates is a man', and C might be 'Socrates is mortal'). Then it follows that A entails $\bar{B} \vee C$. (E.g., it follows that 'All men are mortal' entails 'Either Socrates is not a man or Socrates is mortal'.) 'Of course it equally follows that B entails $\bar{A} \vee C$; but we need not consider both these consequences for our present purpose. Now suppose that A is a *necessary* proposition. Then (i) it is true, and therefore anything which it entails is true. And (ii) anything which it entails is necessary. Therefore if A & B entails C and A is necessary, it follows that $\bar{B} \vee C$ is necessary, or, what is equivalent, that the conjunction B & \bar{C} is impossible. Now this is equivalent to 'Either B is impossible or C is necessary or B entails C'.

Let us now apply this bit of formal logic to the supposition that the Inductive Postulates are *a priori*, i.e., necessary propositions. It is alleged that the postulates A in conjunction with the instantial propositions B entail the inductive generalisation C. Suppose that the postulates are necessary propositions. Then it follows that either the instantial propositions are impossible or the inductive generalisations is necessary or that the instantial propositions entail the inductive generalisation. Now all these three alternatives are palpably absurd. Therefore we must reject one at least of the premisses which led to them. Now one premiss was that the postulates are *a priori*, and the other is that the postulates together with the instantial propositions entail the inductive generalisation. So there is no hope in this direction of justifying inductive generalisations as deductive inferences from instantial propositions and general postulates about nature.

It is even more obvious that the other alternative, *viz.*, that the postulates are themselves inductive generalisations, leads to nothing but a vicious circle or a vicious infinite regress. So we may conclude that no justification of inductive generalisation along these lines is possible.

(To be continued.)

II.—THE EXTRA-LINGUISTIC REFERENCE OF LANGUAGE (II.).

By EVERETT W. HALL.

"It is easy to assent to the statement 'in the beginning was the word'. This view underlies the philosophies of Plato and Carnap and of most of the intermediate metaphysicians."—BERTRAND RUSSELL.

II. DESIGNATION OF THE OBJECT-LANGUAGE.

"'IGLOO'" Carnap assures us, "means (designates) house."¹ A colleague of mine, of Carnapian persuasion, illustrates this principle further: "'Chien' designates dog", and more generally, "'A' designates A". The point of Russell's bon mot quoted at the head of this page is obvious.

How is a word or sentence about extra-linguistic matter of fact related to the matter of fact it is about? It is simple to say that the former designates the latter, and that it can do so through certain associations set up in the speaker's earlier history. But, interestingly enough, to say anything like this requires the use of words about words (at a higher semantical level than those we are talking about). Furthermore, what the words we are talking about refer to does not enter our sentence *in propria persona*, but only as referred to by other words (such as 'what the words are associated with through the speaker's earlier experience'). So we have, 'Igloo' designates the matter of fact with which 'igloo' has come to be associated in the speaker's experience. Why balk, then, at "'Igloo' designates house" or even "'A' designates A"?

Yet we do balk for we object to being landed in the lingua-centric predicament. We find we cannot say anything about the relation between language and matter of fact. In "'A' designates A" we have a metalinguistic statement which by its form indicates or reflects the relation of designation. But this form amounts to a correlation of the name of an expression with an expression. Can it then be taken to reflect the relation of designation between an expression and something extra-linguistic?

Now, my feeling is that, by stressing the pure and the formal character of the semantics he is presenting, Carnap means to rule out all consideration of the designation of zero-level ex-

¹ *Introduction to Semantics*, p. 13 (page references to Carnap, unless otherwise specified, will be to this book).

pressions, *i.e.*, of any expressions save names of expressions. However, I may be wrong here. For, an arbitrarily constructed semantics (as contrasted with a descriptive semantics) might attempt to reflect a common relation (designation) to be found obtaining between (1) names of expressions and those expressions and (2) expressions and extra-linguistic designata. This might even be formal in character, in one sense, namely, in that we do not attempt to specify any definite designata of any specific zero-level terms, but deal only with the general nature of any such relation, and, furthermore, in that this general nature is reflected in or similar to a symbolic structure by which we represent designation as obtaining between a name of an expression and the expression named. For example, in "*A* designates *A*" we have a paradigm of a correlation (between an expression and its designatum); it may be supposed that we arbitrarily decide that if, but only if, a similar correlation holds between *A* and some matter of fact will we allow that *A* designates that matter of fact. It is possible, I say, that Carnap means to accept this view. He may mean to hold that "*A* designates *A*" reflects, formally, the relation of designation at all levels, and thus that although it cannot occur at the zero-level (it is a metalinguistic statement), yet it somehow shows us what we must have if a zero-level term is to designate an extra-linguistic fact. If this be Carnap's view, then he is not (supposing the view tenable) in the lingua-centric predicament, *i.e.*, he is not in the position of saying that only names of expressions (never expressions which are not names of expressions) stand in the relation of designation to something else, or conversely, that only expressions (never extra-linguistic matter of fact) can be designata. But, though there seem to be grounds for saying that this is his view, I do not believe that it is. This leads us to a somewhat more careful presentation of his position.

Pure semantics, Carnap tells us, should be clearly distinguished from descriptive semantics (pp. 11-12). The latter investigates the designation of symbols in historically given languages; it is factual. Pure semantics is concerned with arbitrarily constructed systems. By this Carnap seems to mean that for pure semantics '*designates in S*' (or '*true in S*') is wholly a function of rules in *S* (pp. 12, 24 ff.), and furthermore, that rules in *S* are arbitrary in the sense that they presuppose nothing about matter of fact. It therefore seems clear that what any expression in *S* designates or whether any sentence in *S* is true can be fully decided (in pure semantics) simply by a knowledge of *S*, *i.e.*, without any knowledge of matter of fact. Therefore (I

conclude), pure semantics cannot say anything about the designation or truth of any expression at zero-level, for to do so would involve an assertion (however general and abstract) about matter of fact as well as about language, and would therefore be descriptive.

This conclusion (though explicitly mine) seems to be fairly drawn; that is, Carnap seems to accept it. He tells us that the rules for designation in any (pure) semantical system, S , ultimately rest on enumeration of the form, " a designates a " (where both " a " and ' a ', " a " being the name of ' a ', occur in S).¹ That is, designation is wholly a relation *within* S , therefore within a language (semantical) system, therefore not a relation of language or linguistic elements to extra-linguistic fact.

It might be thought that there is an exception to this conclusion in Carnap's wider definition of 'synonymous with' (where terms in different systems may be synonymous) (D12-2, p. 55). This definition amounts to saying that terms in different semantical systems are synonymous if they designate the same entity.² That is, it would seem that here 'the same entity'

¹ In the simple system S_a , 'designates' is defined: " a_i designates (an entity) u in $S_a = Df$ a_i is the first and u the second member in one of the following pairs: (a) ' a ', Chicago; (b) ' b ', New York; (c) ' c ', Carmel; (d) ' P ', the property of being large; (e) ' Q ', the property of having a harbor" (p. 32). Here the important thing to note is that *the designata*, as well as the expressions designating them, are said to be *in* S , therefore are themselves expressions. No way of determining the designation of *these* expressions (which are not names of expressions) is given. It is apparently no problem for pure semantics. This is borne out by Carnap's definition of an adequate definition of designation in any S (D12-B, pp. 53-4). The key idea is that designation is to obtain between two expressions in a metalanguage when the first is the name of an expression of which the second is a translation. This is simply a generalized (and technical) formulation of "'Igloo' designates house". "Igloo" names a word translated by 'house'. But what is named and translated is a linguistic expression. No extra-linguistic reference of language is considered.

² D12-2 reads: " \mathcal{A}_i in S_m is synonymous with \mathcal{A}_j in $S_n = Df$ \mathcal{A}_i designates in S_m the same entity as \mathcal{A}_j in S_n " (p. 55). This apparently puts us in a new position as regards a definition of designation. Instead of being forced into the form "'Igloo' designates house" we can apparently now say, "'Igloo' designates the same entity as 'house'." This latter form suggests more strongly than the former the possibility of an extra-linguistic designatum. But this is no more than a psychological suggestion, due to the fact that 'Igloo' and 'house' are not in this case names for the expression 'the same entity as', and thus it might be supposed that in them we have expressions which are not names of an expression or expressions, which nevertheless do designate something.

has an extra-linguistic reference, since we have no reason to suppose there is any common expression in the two systems. Carnap's example is: 'Gross' in German designates the same entity as '*P*' in his S_3 , viz., large. But here large must be a word (in English) not a property of fact. For, 'designates' in "'Gross' designates large" and "'*P*' designates large" is defined by pairs of expressions ('Gross', large; '*P*', large) the first members of which are names of expressions of which the second are translations, hence the designatum in each (large) is an expression.¹

Thus in "'Igloo' designates the same entity as 'house'" we have an abbreviation of "'Igloo' designates an expression which is also designated by 'house'", i.e., "'Igloo' and 'house' are both names of a certain expression" (where the expression named is not otherwise named than by 'igloo' and 'house'). This would be the case if, for example, both the following are assumed: "'Igloo' designates house" and "'House' designates house". So again we find that only names of expressions are allowed to designate, and they can only designate expressions.

Let us consider truth. Says Carnap: "We use the term ['true'] here in such a sense that *to assert that a sentence is true means the same as to assert the sentence itself*; e.g., the two statements 'The sentence "The moon is round" is true' and 'The moon is round' are merely two different formulations of the same assertion" (p. 26). This serves to suggest that, in investigating 'true', pure semantics is concerned with what a sentence at zero-level asserts, that is, with the relation of zero-level sentences to extra-linguistic states of affairs. This *may* be what Carnap means, but further investigation seems to suggest that it is not; that he means that pure semantics, in studying truth, is confined to propositions asserting the (truth-)equivalence of sentences at different levels. He defines an adequate definition of 'true' essentially as follows (D7-A, p. 26): a definition of 'true' is adequate if in accordance with it 'true' can be pre-

¹ He says, "'Gross' in German is synonymous with '*P*' in S_3 because Des_0 ('gross', large) ['gross' designates large in German] and Des_{S_3} ('*P*', large) ['*P*' designates large in S_3]" (p. 55). That is, 'the same entity' in this example designates a word ('large') in English. (Carnap uses English as his informal metalanguage throughout his book.) This is borne out by the following consideration. If 'designates' in D12-2 is to satisfy the requirements of D12-B for an adequate definition of designation, there must be an M (in this case, English) in which the designatum of every term which in D12-2 is said to designate must occur as a translation. Hence 'the same entity' of D12-2 must have an expression in an M (not an extralinguistic fact) as its designatum. For, extra-linguistic facts cannot be translations (nor can they be translated).

dictated of ' p ' (the name of a sentence) if and only if p .¹ And he gives a general definition of 'true' for any semantical system which amounts to saying that ' p ' is true if and only if p (D12-1, p. 50).² But what does 'if and only if p ' mean? Carnap gives no answer, and I take it holds that pure semantics does not investigate such matters. That is, when he says that ' p ' is true means the same as p , he is saying not that we can predicate truth only when we know the relation of a zero-level sentence to fact, but quite the opposite, that we can properly predicate truth on the basis of linguistic (semantical) rules alone. Thus if our system contains p it is proper to assert ' p ' is true.³

Continuing, let us note what Carnap says about "absolute concepts" and their relation to "semantical concepts" (§ 17). Carnap distinguishes between semantical concepts which are attributed to the relation between expressions and their designata (e.g., designation) and those which are attributed only to the expressions (e.g., truth). Clearly these latter are always relative to a language (whether they can be predicated of an expression is determined by the rules of the language).⁴ But it is possible to escape this dependence upon language by inventing corresponding absolute concepts. This is done through convention 17-1; which, essentially, says that a semantical property can be predicated of an entity in an absolute way (without reference to a language) if it can be predicated in a relative way (i.e., in the language involved) of every expression designating that

¹ It is not to be argued that ' p ' here refers to a proposition as distinct from a sentence (cf. below, p. 31). For he allows us to substitute a sentence for ' p '. Thus in various sentences in text above, a variable name for sentences (e.g., ' \mathcal{S} ') could be substituted for ' p '.

² The definition is: " \mathcal{S}_i is true in $S = Df$ there is a (proposition) p such that $\text{Des}(\mathcal{S}_i, p)$ and p ." ' $\text{Des}(\mathcal{S}_i, p)$ ' means that ' \mathcal{S}_i ' is the name of p , i.e., ' p '. Thus a sentence is true if it is the name of a proposition, and the proposition (is true).

³ This is clear in his statement of truth-conditions for his examples (S_1, S_2, S_3 , and S_4) (pp. 22-33). E.g., in S_2 the truth condition is: "A sentence pr_i (in _{i}) is true if and only if the designatum of in _{i} has the designatum of pr_i (i.e., the object designated by in _{i} has the property designated by pr_i)" (p. 24). The statement in parenthesis is a little misleading. In place of 'object' 'object-sign' would be less misleading, and in place of 'property' 'property-sign.' For "German letters are used as signs of the metalanguage designating kinds of signs or expressions of the object language" (p. 19). Thus we may translate the truth-condition for S_2 wholly into English: "A sentence (in the metalanguage) asserting that a certain object-sign has a certain predicate-sign attached to it is true if and only if (in the object-language) the object-sign has the predicate-sign attached to it."

⁴ I do not mean to deny this of the former sort; Carnap is here not concerned with them.

entity in any language.¹ Thus he defines the absolute use of 'true': a proposition is true (without reference to a language) if every sentence designating the proposition is true in the language system in which it occurs.² This he thinks allows us to define 'true' (in its absolute use) in the simple form: (D17-1) "(a proposition) p is true = $Df\ p$ " (p. 90).

Let me make two remarks here. First, the important matter for us is that in absolute concepts we may seem to be dealing with properties that can be predicated of extra-linguistic entities. Thus in Convention 17-1 we have 'an entity' (' u '), in D17-B, 'a proposition' (' p '), which designate entities that can be designated by expressions in different (in "any") semantical systems. Thus the designata of ' u ' and ' p ' might be thought to be extralinguistic (we can slough off reference to designation of them in semantical systems, as is done in D17-1). This, however, would clearly presuppose that expressions in different language systems can be synonymous in the sense of designating the same extra-linguistic entity, and is thus subject to the criticism of that assumption we noted above, namely that it violates Carnap's restriction of designation to pairs of terms, the first being the name of an expression of which the second is a translation (since extra-linguistic facts cannot be translations or translated).⁴

My other remark in this connection is simply the observation that "absolute concepts do not belong to semantics" (p. 90). I do this not to suggest that a discussion of them is out of place in a book on semantics (Carnap includes syntax and relations of semantics to syntax in his book). This would be a mere quibble over the title of a book. My question is: If absolute concepts do not belong in semantics, where do they belong? Carnap's only answer (that I have discovered) is: Absolute concepts belong "to the non-semiotical part of the metalanguage (or to the object-language)" (p. 89). This cryptic statement seems to rule them out of the whole sphere of language-investigation (which is divided into pragmatics, semantics, and syntax, the

¹ "A term used for a radical semantical property of expressions will be applied in an absolute way (i.e. without reference to a language system) to an entity u if and only if every expression \mathcal{A} , which designates u in any semantical system S has that semantical property in S " (p. 89).

² (D17-B): " p is true = Df for every [semantical system] S and every [sentence] \mathcal{E} , if \mathcal{E} designates p in S , then \mathcal{E} is true in S " (p. 90).

³ ' p ' is here to be contrasted with ' \mathcal{E} ' in not referring to a linguistic expression, e.g., a sentence. Cf. below.

⁴ That is, D12-2 would violate D12-B if "the same entity" designates anything extra-linguistic.

name for the whole being 'semiotic') (§ 4), yet retains them as "part of the metalanguage (or . . . the object language)". I confess I do not see what this means. Possibly Carnap's rather narrow definition of 'pragmatics' has left an area of semiotic unclaimed by its three divisions: viz., aspects of the occurrence of language and its designata which do not involve a user of the language. But this suggestion isn't very hopeful for clearly not only do absolute concepts designate, but it is precisely with their designata that Carnap is here concerned. Thus it would seem they belong to semantics. It might be suggested that they belong to descriptive (versus pure) semantics. But since they involve no dependence upon language, this seems ruled out. There is another alternative, however. Carnap says they may be part of the object-language. This might mean that in the study of absolute concepts we are concerned not merely with the relation of names of expressions to those expressions but also with the relation of zero-level expressions to matter of fact. If so, Carnap's exclusion of absolute concepts is simply another expression of the position that semantics cannot deal with the designation or truth of zero-level expressions.

It might seem that Carnap has escaped from the lingua-centric predicament in his treatment of the "L-range" of a sentence and his concept of a "real state of affairs" (§ 18). An L-range of a sentence consists of all the possible states of affairs the sentence allows. The L-range of 'My pencil is blue or red' is greater than, and wholly includes, the L-range of 'My pencil is blue'. Thus a sentence's L-range is independent of matter of fact; it is determined by the semantical rules of the system in which the sentence occurs. But the real state of affairs is not. It apparently is a matter of fact, and can be determined only by observation (*cf.* p. 103).¹ Since Carnap, in this discussion, defines 'true' in terms of 'real L-state'² it would seem that here semantics transcends language and thereby escapes the lingua-centric predicament.

I am not at all sure, however, that this is the case. In the first place, Carnap calls the real state (as he does all the possible states) a "proposition". As we shall see presently, though Carnap in this book clearly distinguishes a proposition from a

¹ His definition (D18-A8) is as follows: " rs (the real L-state with respect to S_s) = Df the p such that p is an L-state and true" (p. 105). Here ' p ' designates a state of affairs, a distribution of properties and relations amongst objects, not a sentence. *Cf.* below.

² "Generally speaking, a sentence \mathcal{S}_i is true if and only if one of the possible states of affairs in $Lr\mathcal{S}_i$ [the L-range of \mathcal{S}_i] is the real one" (p. 96).

sentence, he apparently does not mean to identify it with matter of fact. In the second place, there are Carnap's suggestions (§ 19) for developing extensional systems analogous to his system based on the concept of L-range by replacing the elements of the latter (propositions) by something corresponding to them. He presents two sorts of procedures: those in which the elements are intra-linguistic (state-descriptions and sentences) and those in which the elements are extra-linguistic (state relations [relations between properties and individual objects] and correlations of extension [relations between properties and classes of objects]). Now at first sight the admission of these second procedures (setting up systems with extra-linguistic elements) would seem to strengthen the impression that Carnap has escaped from the lingua-centric predicament. But let us note more carefully what the procedures consist in. There is a semantical system whose structure is determined by the concept 'L-range'. Other semantical systems are then formed by replacing elements of an L-range by something else. What is meant by 'replacing'? Clearly putting certain *symbols* consistently in the place of certain others. He doesn't put actual relations of properties to things in place of symbols; and the resultant is a semantical system (a language), not an extra-linguistic state of affairs. So, *both* sorts of procedure referred to replace symbols by symbols (never symbols by extra-linguistic fact, nor fact by fact). Thus Carnap's heading "Procedures of the second kind: the elements of L-ranges are extra-linguistic" is misleading. Instead of 'are extra-linguistic' it should read 'are symbols whose designata are extra-linguistic' or 'are symbols of the zero-level'. Thus again we seem to reach the conclusion: semantics cannot include statements about the designation of zero-level expressions.

But we must consider further the concept, 'real state'. I think Carnap's main purpose in introducing this concept is anticipatory to (or at least analogous to) his treatment of F-concepts (§ 21), so what I shall say about the latter can be considered to apply to the former. "If the L-terms with respect to a semantical system *S* are defined in such a way that the requirement of adequacy . . . is fulfilled, then the L-determinate sentences . . . are those whose truth-values can be determined on the basis of the semantical rules alone. For the other sentences, the rules do not suffice; we must use some knowledge about something outside of language, which we may call knowledge of facts. Therefore the sentences which are not L-determinate have factual content, i.e., they assert something about facts,

namely those facts upon which their truth-values depend" (pp. 140-1). Surely here Carnap has transcended language and come to grips with the relation of language to fact! But let us look more closely. He says, "the sentences which are not L-determinate have factual content". This hints that 'factual' is to be defined simply as the non-logical in a semantical system. This is fully borne out by his formal definitions.¹ D21-1 tells us that if a sentence occurs in a semantical system and its truth-value is not determined by the semantical rules of the system, then it is factual. And that it is factual is itself the consequence of the semantical rules. Thus, to determine what sentences (in S) are factual requires only a knowledge of the semantical rules of S , it is wholly determined intra-linguistically. The study of it does not carry us beyond language. If it be said that we cannot determine what truth-value a factual sentence has by an investigation of semantical rules alone, the proper reply is that semantics (according to Carnap) does not investigate this.

I conclude that there is pretty good evidence that Carnap has not escaped the lingua-centric predicament (appearances to the contrary notwithstanding). Now it might be said that Carnap means to embrace the other alternative I suggested above (p. 26). That is, that instead of saying that semantics cannot, in its investigation of designation, say anything in general about the designation of expressions but only about the designation of names of expressions, Carnap means to hold that designation is the same relation, whether holding between names of expressions and those expressions or between expressions and matter of fact, and that hence, though semantics is forced to use the form " A designates A ", yet it uses this form to refer to designation by expressions which are not names of expressions as well as to designation by names of expressions. I do not find Carnap saying anything like this. But it is a possible interpretation of his position. I think, however, it is one which can be easily disposed of. First, it would clearly imply that though there are semantical propositions that could be applied (by suitable translation) to the relations between language and fact, semantics could not know this or state it. Second, it makes an assumption that is highly improbable (if

¹ D21-1 reads: " \mathcal{E}_i is . . . factual (in S) = $Df \mathcal{E}_i$ is not L-determinate" (p. 141). D21-2 reads: " \mathcal{E}_i is F-true (in S) = $Df \mathcal{E}_i$ is true but not L-true" (p. 141). And D21-3 reads: " \mathcal{E}_i is F-false (in S) = $Df \mathcal{E}_i$ is false but not L-false" (p. 141), and T21-10 reads " \mathcal{E}_i is factual if and only if \mathcal{E}_i is F-true or F-false" (p. 141). This procedure is generalized for all F-concepts, cf. p. 142.

not clearly false). For it seems quite obvious that the relation of designation (and others, as truth, involving designation) cannot be wholly the same when obtaining between expressions in a metalanguage as when obtaining between a zero-level language and extra-linguistic fact, and therefore the linguistic devices for designating it should not be the same in the two cases. For a metalanguage can include expressions named as well as the names of such expressions, and thus the designata as well as that which designates; therefore the truth of a metalanguage-sentence can be wholly determined by a knowledge of the metalanguage.¹ But no zero-level language contains its designata. Therefore it is impossible to determine the truth of any sentence at zero-level by a mere knowledge of the zero-level language. This agrees with Carnap's position: (pure) semantics is wholly non-descriptive; the truth of its sentences is wholly independent of matter of fact (*i.e.*, not merely of this or that fact, but of even very general and abstract aspects of fact).

As a matter of fact, I think Carnap's reaction would be quite different from that envisaged by this suggestion. I think he would simply admit that semantics cannot escape the linguistic predicament. Semantics is arbitrarily or by conventional division of labour confined to language (in its designative aspects) as abstracted from all matter of fact, he might claim. The study of the relations of language to extra-linguistic fact he might put in descriptive (*vs.* "pure") semantics, in pragmatics, or perhaps outside semiotics entirely. Such a position, it seems to me, is quite unobjectionable if one thing is made perfectly clear (which Carnap does not make perfectly clear and explicit in his recent book): On this view (pure) semantics cannot investigate the designation of terms (the truth of sentences, etc.) occurring at the zero-level. It not merely cannot

¹ It must not here be objected that semantical sentences in a metalanguage are about the designata of expressions in the object-language as well as about the expressions themselves, and that only the expressions are contained in the metalanguage. For, the hypothetical position I am refuting agrees with my analysis of Carnap in denying that semantics can deal with extra-linguistic designata. And anyway, I would have the following case to use: Take a semantical sentence at second level, which asserts a relation of designation holding between a term at first level and one at zero-level: *e.g.* "'A' designates A" (or to be wholly unambiguous: "A" designates 'A', 'p' designates a predicate!). The truth of *this* could be wholly determined by a knowledge of the second level metalanguage (for higher metalanguages include all below them). But the truth of an object-level sentence can never be determined by a mere knowledge of the object-language.

determine what is the designatum of a particular term; *it cannot say anything whatever about designation at this level*. It cannot even assume that there is an absolute zero level, where terms designate extra-linguistic fact, for to do so would be to take a position about a matter of fact. It can construct (arbitrary) semantical systems in which language-levels are numbered, and in which 'zero' occurs, as the name of a level. But it must treat such zero-levels as relative to the semantical system in which they occur. Whether such a zero-level language could ever be interpreted as a language having extra-linguistic designata would be a question wholly outside the domain of pure semantics. The attachment to empirical fact of such a semantical set of languages would be a matter for some other discipline to consider. Likewise, when such a pure semantics says that ' p ' is true means the same as p , and thus is able to define 'true' at higher levels in terms (essentially) of true at lower levels, this must not be taken to mean either (1) that semantics can determine the truth-value of sentences about extra-linguistic fact (Carnap points this out) or even (2) that semantics has the right to assume that there are true or even true-or-false sentences about extra-linguistic fact. Carnap's factual concepts are simply any semantical concepts that are not L-determinate. Whether the terms of which they can be predicated can be interpreted as designating extra-linguistic fact is a matter wholly beyond the purview of semantics (in this use of 'semantics').

As I say, I see no objection to this use of 'semantics' if one is clear and consistent in it. But in this usage, semantics has *nothing whatever* to say about our problem: the relation (particularly of designation) between language and extra-linguistic matter of fact. How is it possible for a word or sentence to refer to extra-linguistic matter of fact? Pure semantics gives us no answer. On the other hand, we must not be satisfied with the merely psychological answer: A word can refer to a non-verbal fact because it has been associated with the latter in the speaker's earlier experience. For, this answer begs the question, being wholly composed of words some of which, it is supposed, refer to extra-linguistic fact. Perhaps the question can be put more clearly: How is it possible to say anything whatever about the extra-linguistic reference of language, for whatever you say will be confined to language?

My answer is simple. It is possible to say things about the extra-linguistic reference of language because (historical) language includes a class of symbols I shall call "empirical ties". Roughly,

empirical ties are the denotatives: demonstrative pronouns ('this', 'that'), relative adverbs ('here', 'now'), also symbols often not called linguistic, such as gestures (pointing), etc. Such an empirical tie as 'this' serves to attach language to matter of fact. "What do you mean by 'igloo'?" "'Igloo' designates house." "But what do you mean by 'house'?" "This." This attaching of language to empirical fact, this empirical orientation of language, is possible because of a peculiarity of empirical ties. An empirical tie is both a symbol and (a part of, or in the context of) the designatum of that symbol. Like all symbols, empirical ties are matters of fact as well as of language, but they are peculiar in that their linguistic (symbolic) function is through or by means of their factual occurrence.

To state this more clearly, let me use Carnap's distinction between sign-event and sign-design (§3). The sign-design is what is usually meant when we use such words as 'symbol', 'word', 'sentence'. It is the form or structure common to a set of actual occurrences (sounds, marks, gestures) whereby they function symbolically to designate the same designatum or the same kind of designatum;¹ it is thus abstract. A sign-event is any concrete occurrence of a sign-design. In "'Igloo' is a noun", 'Igloo' designates a sign-design. When we say 'Igloo' is to the right of 'in' and to the left of 'is' in a certain set of marks (designated by 'the preceding sentence'), 'igloo' designates a sign-event.

It is of course important (as Carnap insists) that when we deal with signs we should always be clear as to whether we are concerned with them as designs or as events. Moreover, in most investigations of language (Carnap says in all cases in syntax and semantics) we are wholly concerned with signs as designs: we completely ignore their concrete occurrence as events. But, it is my contention, we must recognize the function of signs *as events* (in designation of fact by zero-level sign-designs) if we are ever to escape the lingua-centric predicament.

The peculiarity of an empirical tie, such as 'this', lies in the fact that its occurrence is part of its designatum (or a distinguishing mark in the context of its designatum). That is, in the case of empirical ties the common sign-design of a set of sign-events does not designate a single designatum (as with proper names) nor designata of the same sort (as with common

¹ This would have to be modified so as to allow us to say that, e.g., 'igloo' as spoken and as written is the same word. We might, e.g., say that two different sign-designs are instances of the same word. But this is unimportant for the argument.

names or descriptive predicates). It is radically ambiguous. In fact, the common sign-design simply is an indication that we have here an empirical tie that functions in a certain fashion to designate a designatum unique for each occurrence of the sign as an event. That is, the sign-design itself does not designate at all: it simply indicates that an event (any event embodying it) is a sign-event, and as such does designate. What is designated by such a sign-event is something in the existential context of the sign-event. If the empirical tie occurs alone (unaccompanied by other signs) then what it designates within its total immediate existential context is wholly ambiguous. But conjoined with other signs, it functions to limit their designata to the case or cases present in its immediate existential context. 'This' or 'look' is wholly ambiguous within the region of its immediate existential context. But 'this book' or 'the colour of this book' is not. "This book" means "The book in the immediate existential context of a particular utterance I (later and by printed marks) name 'this'". This, however, supposes that other signs (sign-designs) have meaning (are empirically attached) independently of a given occurrence (sign-event) in conjunction with an empirical tie. In order that this be so, however, those other signs (sign-designs) must have been attached (or must now be attachable) to matter of fact through differential existential conjunction (sign-events) with empirical ties.

Let me illustrate by a hypothetically simplified case. Suppose I have a red sphere, a blue sphere, a red cube, and a blue cube. Suppose I have a subject properly conditioned so that at least one empirical-tie-design is present in this vocabulary (such as pointing, or 'look' or 'here-now'). Then the attachment of other words becomes easy. I present the red sphere and say "Look! red" then the red cube and say "Look! red", then the blue sphere and cube, in each case saying "Look! blue". Similarly for 'sphere' and 'cube'. By multiplying the differential existential conjunctions it becomes theoretically possible to tie all symbols in a zero-level language to these factual designata. This illustration may be misleading, however. I do not mean to present an hypothesis in genetic psychology. What I am concerned to assert is that however we may have learned our empirical language, the extra-linguistic reference of that language can be itself stated in that language only by the use of empirical ties. Whenever challenged as to the meaning of a zero-level term (such as a descriptive predicate), our only recourse is to a statement of the form: "I mean by

'- - -' this".¹ Such statements carry us outside the sphere of "pure" (non-descriptive) semiotics. For, clearly the 'this's' in them designate matter of fact. Furthermore, they must be uttered or written in the proper extra-linguistic, existential context. If it be said that this throws the consideration of them into psychology or pragmatics, I have no objection. I would simply insist that without them no consideration of designation at the zero-level, of the meaning of words about extra-linguistic fact, is possible—no matter how abstract or general.²

My view may be clarified by a comparison of my "empirical ties" with Russell's "ego-centric particulars" (Ch. vii).³ Clearly the general context of thought is the same. We are both concerned with the problem of how it is possible to say, in language, anything about the extra-linguistic reference of language. Furthermore, we have in mind the same general group of symbols ('this', 'I-now', 'here', etc.). I see several differences, however. First, the names are different. I object to 'ego-centric' for as I see it no reference to a self is essentially involved in an empirical tie. This is not so obvious with gestures or utterances as with written signs. Suppose I write: "Up to this point this paper is composed of such and such a number of words". In this sentence, two 'this's' occur. In neither is there involved any reference to myself. But Russell himself recognizes this, so the difference is one of names only.

A more significant difference arises in Russell's attempt to classify ego-centric particulars. Russell asks whether ego-centric particulars are proper names or (tacit) descriptive predicates. As I see it, a proper name is a sign-design designating a single designatum, whereas a common name or descriptive predicate designates any of a number of designata of the same kind.⁴ Thus proper names and descriptive predicates are not sign-events but sign-designs, and an investigation of their meaning can properly ignore their occurrence. But an empirical

¹ More accurately: "I mean by '- - -' this₁, this₂, this₃, . . . (in contrast with this₁, this₂, this_{n+1}, . . .; and this₂, this_m, this_{m+1}, . . .; and . . .)".

² I might point out an interesting parallel here to 'designation' as conceived in pure semantics. In pure semantics 'this' designates this. On the view I present, 'this' (as sign-design) designates the existential region of this (as sign-event).

³ References are to *An Inquiry into Meaning and Truth*.

⁴ It does so through a property which itself has a proper name—the substantive form of the descriptive predicate: thus 'red' as an adjective is a descriptive predicate, 'red' as a noun is a proper name.

tie is not strictly a sign-design, therefore neither a proper name nor a descriptive predicate. It is in a sense a sign-design, but the design has, strictly, no meaning; it is a way of indicating that a particular event is a sign-event, and has just its own unique reference (contrasted with other sign-events having the same design).

It is Russell's treatment of ego-centric particulars as sign-designs, and thus as necessarily either proper names or descriptions, which gets him into his serious difficulty (which I believe my empirical ties escape). "The word 'this' is one word, which has, *in some sense*, a constant meaning. But if we treat it as a mere name, it cannot have in any sense a constant meaning, for a name means merely what it designates, and the *designatum* of 'this' is continually changing. If, on the other hand, we treat 'this' as a concealed description, *e.g.*, 'the object of attention', it will then always apply to everything that is ever a 'this', whereas in fact it never applies to more than one thing at a time. Any attempt to avoid this undesired generality will involve a surreptitious re-introduction of 'this' into the *definiens*" (p. 136).

With Russell's solution of his problem I disagree wholeheartedly. It is that 'this' "is a word which is not needed for a complete description of the world" (p. 141). In contrast, I hold that 'this', or some other empirical tie, is needed¹ for any description of the world, however incomplete. Russell's argument, if I follow it, is that the difference in two 'this's' (or as he puts it, between 'this is' and 'that was') lies not at all in their meaning but wholly in their causation. The only difference between 'this is a cat' and 'that was a cat' lies in the fact that between the stimulus and utterance in the first case lies a minimal causal chain, but, in the second, there is a longer chain: there is no difference of meaning in these sentences (therefore no difference in meaning between 'this is' and 'that was'). Let us apply this to the radical ambiguity of 'this' (Russell does not do so, though this ambiguity is his basic problem). It would seem to require that when I say 'this is a cat' at place-time₁ and 'this is a cat' at place-time₂, there is no difference of meaning but only of causation. But this is absurd, for it is possible for one of these utterances to be false, the other true. But suppose Russell means simply 'this is a cat' uttered at place-time₁ is true and 'that was a cat' uttered

¹ I do not mean that every descriptive sentence actually contains an empirical tie, but rather that its empirical attachment can be shown only by the use of an empirical tie.

at place-time₂ is likewise true. This would simply mean that as 'this' shifts in designation, 'that' (and other ego-centric particulars) do so likewise. Thus the 'this' of place-time₁ may have the same designatum as the 'that' of place-time₂. But if this is what he means, then he is simply mistaken in denying difference of meaning to 'this' and 'that'. Suppose they are both taken relative to the same existential context. Then of 'this is a cat' and 'that was a cat' one may be true and the other false; therefore they cannot have the same meaning. The case would be exactly similar for 'now' and 'then', 'to-day' and 'yesterday', etc. Of course it is possible to shift your point of temporal reference so that 'to-day' and 'yesterday' may be made to designate the same day, but when uttered in the same existential context their designata are different. I conclude that Russell has not shown that we can dispense with 'this' in describing the world, that 'this' and 'that' do not differ in meaning, nor that 'this's' occurring in different existential contexts have no difference in meaning.

However, such words as 'that', 'then', 'there', 'yesterday', etc., call for a qualification of my account of empirical ties. They are empirical ties, but do not designate the immediate existential context of their own occurrence, but rather some other context. They designate some other existential region in contrast with the one in which they occur. This is possible through a combination of the basic empirical tie with a descriptive element. 'That' (demonstrative) means: in an existential context different from that of its own occurrence as sign-event. 'Then' means: in an existential context temporally preceding that of its own occurrence as sign-event. 'There' means: in an existential context spatially different from that of its own occurrence as sign-event. 'Different from', 'temporally preceding', 'spatially different from', etc., are descriptive relation-terms. Their extra-linguistic reference like that of all descriptive predicates and all proper names, can be shown only by using them in sentences including empirical ties. Thus the class of empirical ties can be indefinitely enlarged by amalgamating the functions of empirical attachment and description in single words or phrases.

Let me conclude this discussion of empirical ties by dealing with three alternatives. They are based on the assumption that what I have called "the empirical attachment of language" is necessary if a language is to have extra-linguistic reference, but they suggest that this can be done by other means than empirical ties. The first is that language can be attached to fact

through the use of proper names. Since a proper name designates an individual, it might be thought that a language can be empirically oriented through the way proper names (especially in conjunction with descriptions) occur in it.

To this I would object, in the first place, that proper names are ordinarily names of things (such as people) having histories, thus including change, and being spread out in space, thus including parts. This immediately opens a vast opportunity for ambiguity in the attachment of descriptive terms to factual properties.¹ This, however, might be avoided by the device of combining proper names with some such term as 'now'—*e.g.*, instead of 'John Jones' we could use 'John-Jones-now'. But this would admit empirical ties into our language. In place of this device we might try to set up a language allowing no ambiguity in proper names. Thus every point in space-time would have its own proper name.² This, however, would run up against a pragmatic difficulty: no human being could learn, in fact could even enumerate, the words required in such a vocabulary. Even this difficulty might, perhaps, be avoided by limiting proper names to some small number of names of space-time points,³ selected so that other space-time points could be referred to descriptively. Granted the possibility of such a language, the basic difficulty (as I see it) remains untouched. Take any one proper name, say '*a*'. '*a*' supposedly unambiguously names a certain space-time point. *What* space-time point? A language without empirical ties could not say. Such a language could say a great deal. It could say that if '*a*' and '*b*' are different proper names, they name different space-time points. It could say that if '*a*' enters a set of different, true sentences, then some space-time point exemplifies a plurality of properties and relations. But just what the extra-linguistic designatum of '*a*' (or any other term in this language) is could never be stated in this language (not through a lack of knowledge as to what '*a*' names, but through a lack of linguistic means). As far as anything this language could say, '*a*' could be the name of *any* space-time point *whatever* (though of only one). This language would virtually say: take your

¹ The name of a property or relation is itself a proper name—*e.g.*, 'red' as a noun versus as an adjective. The designata of such proper names do not change. But the designation is (in most cases) highly ambiguous. *E.g.*, 'red' names any of a set of at least a dozen easily distinguishable colours.

² Likewise, every distinguishable property and relation.

³ And of properties, distributed through various modes, and relations, of various kinds.

choice, within the whole realm of space-time points, as to what is the designatum of any proper name, 'a'. This would amount to a complete and radical ambiguity of 'a'. But if so, 'a' is not a proper name at all. It is simply a mark or utterance which is to obey certain rules of language. It *names* nothing. We find ourselves squarely in the lingua-centric predicament. It might be thought that this unfortunate result can be avoided if it be admitted that the designatum of 'a', where 'a' is taken alone, cannot be determined, but in the total language, with many proper names and descriptive symbols, used differentially, we have a system which unambiguously determines the extra-linguistic reference of each proper name within it. This is a form of the second alternative to which I now turn.

The second alternative would say that the empirical attachment of a zero-level language can be unambiguously fixed through its structure. As Wittgenstein held, the structure of the language shows the structure of the world. If the structure of a language is the same as the structure of the field of its designata, then the extra-linguistic designata of various elements in the language can be determined as the structurally corresponding elements in the realm of fact.

To this I can see several important objections. In the first place, it would involve us not merely in actual, but in logically unavoidable, ambiguity if there are structures in fact which are repeated. Suppose colour-hues and tone-pitches have the same structure (one-dimensional continuum with top and bottom limits), such as is suggested by the translation of one into the other in synaesthesia and colour-symphonies. Then there would be no possible way of referring to hues as distinct from pitches, or vice versa. Now I think it is highly probable that there are repetitions of structure in fact. But even if there are not, there are approximations thereto, so close that for common sense and ordinary experience we may properly speak of different regions of fact as structurally identical. Thus ordinary language should not be able to distinguish these regions. But clearly it does, as the illustration of hues and pitches indicates.

Second, this structural view of empirical attachment could not admit in language at the zero-level any purely syntactical structure, i.e. structure due to the language as language in contrast to what it asserts. This objection need not be based on the assumption that all structure in language is a matter of convention. In fact, it might hold that it is possible to construct a language where no structural feature is arbitrary, yet

that in such a language there are two types of structural features : those required by the nature of what is asserted (fact) and those required by the nature of assertion (language). Even in such a language, however, fixing the empirical attachment of symbols could not be accomplished simply through the structure of the language, for there would be no way of determining what structural features in the language are to have extra-linguistic significance and what not. Simply by a knowledge of the structure of the language, it would be impossible to know that structural features L are of linguistic significance only, whereas structural features F have extra-linguistic significance. However, if we know the structure of the world and also of our language, can we not find that, in some parts or aspects, they are identical, and conclude that these are the F-features of the language, and thus be enabled to fix the empirical attachment of our language by structural correspondence of such F-features and the structure of the world ? I think even here we have an insuperable difficulty. Suppose an F-feature of our language-structure. It then refers to whatever extra-linguistic fact has this same structure (and we here wave difficulties as to repetitive structures). Now our question is, what does 'the same structure' mean ? We have a structure in language. It is to refer to something in fact. To show *what* something (a something which in a metalanguage is said to be identical in structure with the language) it is necessary at some place to indicate that the language refers to fact *having the same structure*. The mere fact that language and fact have a structure in common does not suffice to insure that the language will designate just the fact with such a structure. What any feature or element of any language designates is a matter either of fact or of choice ; there are no *a priori* necessities here. 'Designates' may actually *be* equivalent in certain languages or in all actual languages to 'having a common structure', but to assert that it is is not an analytical proposition. But now if it is simply a fact that the F-features of our language designate fact having the same structure, then no knowledge of these F-features of language is sufficient to determine what they designate, nor can we state what expressions in the language designate simply by using expressions displaying F-features. It would be necessary to attach expressions displaying F-features to fact having the same structure. This can only be done, as I see it, by language elements which, while being linguistic, are also themselves a part of the fact designated.

The third alternative would say that the empirical attachment of a zero-level language occurs through a process of interpretation.

Through a proper correlation of zero-level terms and empirical fact, the terms and facts get to be associated in someone's experience, or someone becomes properly conditioned in his language habits. This correlation it might be said requires no specific terms to indicate it (either in general or in particular), all it requires is a sort of isomorphism between the world of fact and zero-level non-logical terms, such that (the simplest case) for each property in the world there is a unique descriptive predicate, and for each individual entity there is a unique proper name.

The difficulty with this would be that in no language at any level could it be significantly said that zero-level terms are interpreted. To say that they are correlated with fact would have to be done by some correlation of terms with terms. It might be claimed that this intra-linguistic correlation reflects the correlations of language with fact. But even if it be admitted that such an isomorphism does occur, it is impossible to assert it within the limits of any language which itself can only reflect it. The argument here is essentially the same as concerning the second alternative above. Language may actually be correlated with extra-linguistic fact, but to assert this correlation requires more than the fact of the correlation; it requires terms (empirical ties) which are not merely correlated with the facts they designate but are themselves those facts (or parts of them).

I shall conclude this paper with a brief discussion of the relation between sentences, propositions, and facts. Let me begin with a statement of Russell's position. "An assertion has two sides, subjective and objective. Subjectively, it 'expresses' a state of the speaker, which may be called a 'belief', . . . Objectively, the assertion, if true, indicates a fact; if false, it intends to indicate a fact but fails to do so" (p. 214).¹ Thus "sentences signify [or express] something other than themselves [i.e., beliefs, Russell also calls them propositions], which can be the same when the sentences differ" (p. 237). "When I say 'snow is white', . . . I express a belief. But I am not asserting that I have a belief; I am asserting the object of the belief" (p. 220). This last quotation is the key one for my purposes.² But before bringing it out, I wish to draw a distinction which will help to keep the main issue clear. Russell says that a sentence expresses a belief, by which he clearly means something subjective and psychological (such as an image).

¹ References are to *An Inquiry into Meaning and Truth*.

² Russell uses his distinction of what a sentence signifies from what it indicates to help him formulate a criterion of meaningful (as against meaningless) expressions. This is irrelevant to my purpose.

This raises unnecessary psychological complications. *E.g.*, if 'proposition' designates belief, then Carnap's truth condition, '*p*' is true if and only if *p*, does not universally hold, for "'*p*' is true" and '*p*' could not be equivalent. '*A* believes *p*' is not equivalent to '*A* believes "*p*" is true'. *A* may not be Carnap, but a quite non-logical chap. The truth of statements about what *A* believes must be determined empirically.

It may be the case that propositions as extra-linguistic entities that are true or false occur, and only occur, as beliefs. But if so, their *occurrence* may be abstracted from. But it also is possible that they do not occur at all, that they are (abstract) properties (the assertion or designation) of sentences. In either case, it is possible to deal with *the nature only* of propositions, which I take to be their assertiveness (or designativeness). In this respect, propositions are on a par with sentences. Though sentences do occur (as utterances or "mounds of ink"), it is possible to treat them simply as sign-designs.¹ Furthermore, just as it is possible to make significant statements about sentences that have not occurred but might occur (in a given language), so it is significant to talk about propositions that are possible, though they have never occurred as psychological beliefs. If we do treat propositions thus abstractly, as simply assertions that - - - or designations of - - -, then anything subjective or psychological about their occurrence may be properly ignored. In '*A* believes *p*' we are concerned with a proposition-event, not simply with a proposition. Thus Carnap's truth-condition for propositions may hold strictly for propositions but not for proposition-events.

With this qualification, I think Russell's distinction between what a sentence expresses (or as I shall say, formulates) and what it indicates (or as I shall say, asserts) is legitimate and important. Every (meaningful) sentence formulates a proposition, but it does not assert the proposition. It asserts exactly what the proposition asserts. Now it might be objected that this view requires certain entities (propositions) which should be cut off with Ockham's razor (though Ockham insisted on them, under the name of 'natural signs'). I do not believe the objection is valid. First, it rules out a real possibility, *viz.*, that there are non-linguistic entities (*e.g.*, beliefs) that are true or false. Second, it makes it difficult (though perhaps not impossible) to distinguish between the symbolic (formal or design) features of sentences and their propositional or assertive features (whereby they are true or false). Even though it be admitted

¹ Except in the problem of empirical attachment. See above.

that propositions are not existents in their own right, and that a sentence must designate or assert as well as exhibit a certain spatio-temporal design, it is difficult to keep these two aspects distinct without a terminological distinction.

I suggest the following convention. Suppose we have the sentence: 'This ink is blue'. Let us refer to the fact which makes it true (the fact of this blue ink) by p (without quotes). Let us refer to the sentence (as a sign-design or as involving a sign-design) by ' p '. Let us refer to the proposition ' p ' formulates (or the assertive aspect of ' p ') by $\langle p \rangle$. Then we shall say, ' p ' formulates $\langle p \rangle$, $\langle p \rangle$ designates or asserts p . By a further convention we can deal with sign-designs quite independently of what they designate (syntactics). Thus ' p ' can be considered purely formally. We then have the following distinctions: ' p ' may be interpreted or uninterpreted. When interpreted, it must be the formulation of a $\langle p \rangle$. The distinction of interpreted-uninterpreted is irrelevant to $\langle p \rangle$ and p . When uninterpreted ' p ' is neither true nor false; but when interpreted, it is either true or false. $\langle p \rangle$ must be true or false. p is never true or false. " p_1 ' = ' p_2 '" can signify either that ' p_1 ' and ' p_2 ' both formulate the same proposition $\langle p \rangle$ or that they formulate different propositions with the same truth-value $\langle p_1 \rangle \equiv \langle p_2 \rangle$. It is legitimate to hold (with Carnap, p. 26) that whenever ' p_1 ' and ' p_2 ' are logically equivalent, they formulate the same proposition. One important instance of this would be "' p ' is true" is logically equivalent to ' p '.

It is of course possible that both ' p ' and $\langle p \rangle$ occur at various semantical levels (though not for p).¹ These two hierarchies, however, are to be kept distinct. This is clear if we admit that ' p ', "' p ' is true", "' p ' is true" is true', etc., all formulate the same proposition $\langle p \rangle$, though at different semantical levels.

If space permitted, I should like, in conclusion, to tie the two parts of this paper together by showing that they both advocate extensions of 'object-language'. The first part would have the object-language include certain logical words, such as 'not', 'all' in their radical meaning. The second part would require empirical ties, such as 'this' and pointing, to occur in the object-language. In fact, it contends that only as such empirical ties (which designate through their occurrence) are in the object-language can other words in that language have empirical attachment. Putting these two together, the implication is that the designation of logical words in their radical meaning

¹ There may be an ontological hierarchy of types in extra-linguistic fact, such as properties of properties.

in the object-language can be shown by differential existential conjunction with empirical ties. Since, however, such logical words have no meaning in isolation, the answer to the question, what does 'not', *e.g.*, mean at the object-level? would not be, 'not' means this. It would rather be, 'not . . . ' means this₁, this₂, . . . etc.; 'not - - ' means this_n, this_{n+1}, . . . etc.; etc., where the meaning of '. . . ' and '- - ' can be shown by differential existential conjunction with empirical ties. This crude suggestion must suffice as an indication that, at least in the author's own mind, the two parts of this paper are not in basic contradiction.

However, whatever may be thought of the specific suggestions offered in this paper, it is hoped that the paper may prove a stimulus to a certain type of important but largely neglected investigation of language, falling neither in pure semantics nor in psychology (at least as actually pursued). I refer to the inquiry as to how the designation by object-language expressions of extra-linguistic fact can be significantly formulated in language. We may, of course, be here within the realm of the ineffable. It is the conviction of the present writer, however, that there is nothing ineffable.

(Concluded.)

III.—SELF-CONTRADICTIONARY SUPPOSITIONS.

BY ALICE AMBROSE.

FERMAT made the "conjecture" that for all integers n , $2^{2^n} + 1$ is prime. In his review¹ of M. Lazerowitz' paper,² Professor Alonzo Church says of this: "... since, by calculation, $2^{2^5} + 1 = 641 \times 6700417$, the conjecture is logically impossible, but it would hardly be said to follow that Fermat never made any conjecture on this point".³ His statement is intended as a criticism of the view that self-contradictory expressions are meaningless, since it would seem to follow from this view that any expression in number theory of the form "Suppose p ", where p is shown by a contradictory instance or by a *reductio ad absurdum* proof to be impossible, could express no conjecture at all because it has no meaning. Professor Church holds that expressions such as "for all n , $2^{2^n} + 1$ is prime" have "an intelligible meaning for anyone with a knowledge of elementary arithmetic and algebra"⁴ even though they state something impossible. That this is the case he thinks follows from the fact that they function within expressions of the form "Suppose p ".

Whether or not self-contradictory expressions⁵ are to be classified as meaningful I do not wish to discuss here. In fact I think that this question has no answer, in the sense that it is not a part of the analysis of "' p ' is a self-contradictory expression" that it is either meaningful or meaningless. In any case I shall leave this question aside to discuss the claim Church makes in connexion with self-contradictory expressions " p " of the sort he cites from number theory that if they are meaningless one has made no supposition at all in saying "Suppose p is true", that is, that it is a necessary condition for the existence of a supposition about p that " p " be meaningful. This is the thesis from which he argues to the meaningfulness of self-contradictory expressions. For it is a fact that in mathematics suppositions about self-contradictions are made. Church's thesis, together with this fact, entails that self-contradictory expressions have meaning. Now it is the case that in *ordinary* discourse the use of "conjecture" and "supposition" is so

¹ *Journal of Symbolic Logic*, vol. 5, no. 2, p. 81.

² "Self-contradictory Propositions," *Philosophy of Science*, vol. 7, no. 2.

³ *Journal of Symbolic Logic*, *op. cit.*, p. 82.

⁴ *Ibid.*

⁵ I shall use this term to cover not only expressions of the form $p \cdot \sim p$ but such expressions as "There is a third root to $x^2 = 4$ ".

connected with the use of the word "meaning" that " S supposes p " entails " p has meaning". With any meaningless expression "suppose" could never be sensibly used, for example, we could not sensibly say "Suppose 'twas brillig and the slithy toves did gyre and gimble in the wabe". No supposition is possible, for there is nothing (no meaning) with regard to which a supposition is being made. Hence if the mathematical use of "suppose" is the same as the ordinary one, Church has produced an argument showing Lazerowitz' claim to be wrong. What I wish to show in this paper is that there is a mathematical usage of "suppose" such that statements of the form " S supposes p " do not entail that " p " has meaning. This is not to argue that in connexion with such a usage " p " is meaningless, but only that from the fact that in mathematics suppositions about self-contradictions are made it does not follow that expressions for such self-contradictions are meaningful. Thus, although nothing is thereby offered in defence of the view that self-contradictory expressions are meaningless, Church's reason for saying they have meaning will be shown to be mistaken.

It is tempting to object to Church's argument on the ground that it is impossible to make a supposition with regard to what is self-contradictory, in which case it would no longer be open to Church to argue that self-contradictory expressions had meaning from the fact that a self-contradiction had been supposed. One might agree that if an expression " p " is meaningless, "Suppose p " expresses no supposition, and then go on to maintain that it is in fact the case where p is self-contradictory that no supposition about it can be made. Ordinary usage lends plausibility to such an assertion. For if one were to ask someone to suppose that his house were both white all over and red all over, the reply would be that this cannot be supposed. The same considerations prompting one to make such a reply might be held to obtain in cases where one introduces a *reductio ad absurdum* proof by asking that something self-contradictory be supposed. Now I do not wish to make objection to Church's argument on any such ground, but I shall set out the considerations leading one to hold that in mathematics no supposition with regard to what is self-contradictory can be made, because Church's claim and these considerations are infected by a common error. When it is clear what confusion results in the paradoxical claim that in mathematics nothing self-contradictory can be supposed, I think it will also be clear that the same confusion is responsible for Church's arguing that self-contradictory expressions are meaningful.

It is the very great differences between *reductio ad absurdum* arguments in which "what is supposed" is self-contradictory and those arguments in which it is not which tempt one to hold that nothing self-contradictory is ever supposed. It will therefore be useful for making one see what is wrong with arguments leading to this conclusion to consider two such arguments, A and B, the one from number theory and the other from ordinary discourse:

- A. Suppose there is a rational number $\frac{s}{t} = \sqrt{2}$ (s and t relatively prime).

Then $s^2 = 2t^2$, which implies that s is even: $s = 2n$.

(If the square of a number is even, the number itself is even.)

Since $s = 2n$ and s and t are relatively prime, t is odd.

Since $s^2 = 4n^2$, whence $2t^2 = 4n^2$, or $t^2 = 2n^2$, t is even.

Thus t is both even and odd.

But this is impossible.

\therefore There is no rational number $\frac{s}{t} = \sqrt{2}$.

- B. Suppose that the earth is a perfect sphere.

If this is true, a pendulum will make the same number of swings per second everywhere on its surface.

But the number of swings varies at different points.

\therefore The earth is not a perfect sphere.

The general form of these arguments is as follows: (For the purpose here it is not required that the form of A, which is more complex than that of B, be exhibited in detail).

- A. Suppose p is true.

$p \rightarrow q \cdot \sim q$.

But $q \cdot \sim q$ is impossible.

$\therefore p$ is impossible.

$\therefore \sim p$ is true.

- B. Suppose p is true.

If p , then q .

But q is false.

$\therefore \sim p$ is true.

The formal analogy between these two arguments is clear. If now we delineate their differences we shall see how easily one finds oneself committed to denying that any supposition in A could ever have been made. Each conclusion, $\sim p$, is the formal negative of what one is asked to suppose, but in the one case, B, what follows from all the premises taken together is the mere matter of fact falsity of p , while in the other, A, what follows from the single premise p is its own self-contradictoriness. That the latter does follow *shows* that p is self-contradictory, whereas in B it is only shown that p 's falsity is a *consequence* of certain premises, and the possibility of p 's being true still remains. It is logically possible, i.e. it is not self-contradictory, for the

earth to be perfectly spherical. But it is impossible that there should be a rational number $\frac{s}{t} = \sqrt{2}$. The supposition that the earth is perfectly spherical, *i.e.* a supposition contrary to fact, presents us with no puzzle. It is logically possible for what is in fact false to be true and for what is in fact true to be false, and hence it is unquestionably possible to suppose either one or the other. It might appear to be proper to give an analogous description of the premise of A, "There is a rational number $\frac{s}{t} = \sqrt{2}$ ". This statement might be held to have, in fact, the property impossibility, so that in supposing it to be true one would simply be supposing something contrary to fact. But it is not a matter of empirical fact that p ("There is a rational number $\frac{s}{t} = \sqrt{2}$ ") has this property. Impossibility is a necessary, or intrinsic character of it, *i.e.* it is logically impossible, or inconceivable, that it should not have it.

It is this consideration which tempts one to raise the question, Can one suppose that what is impossible is true? Is this not the same as supposing that a property necessarily belonging to the premise p of A does not belong to it? If p is impossible, can one conceive that it is not impossible—as one seemingly is being asked to do in being asked to "suppose" p is true? One feels impelled to answer that if p could not conceivably be other than impossible then to suppose it is true is itself impossible—in which case we have in A no supposition at all.

A somewhat different characterization of contradictions, which is connected with their characterization as impossible, likewise makes one feel, after having made the deduction A from premise p , that it is impossible one should have done so. This is the commonly accepted description of self-contradictions as "inconceivable". The form of the conclusion in A, namely, $\sim p$ is true, suggests that we have here, as in B, one of two alternative possibilities, that either alternative *could* be the case. But the conclusion of A is necessary; it is a truth to which no alternative is conceivable.¹ That is, its opposite, p , is such that it could not conceivably be the case. We do not know what it would be like for it to be true—it is "logically inconceiv-

¹ C. I. Lewis and C. H. Langford, *Symbolic Logic*, p. 24. It should be clear that in saying this there is no disparagement of anyone's capacities for conceiving. There is no comment here about anyone's capacities as there is in "He can't conceive of a better world".

able". The puzzle arises when we consider what we are being asked to do in being asked to suppose a self-contradiction is true. Are we not being asked to conceive something which is inconceivable? Is this not logically impossible? But if it is logically impossible, then it is logically impossible that a supposition in A has been made.

I think there can be no doubt it is a mistake to think these arguments show that nothing self-contradictory can be supposed. If they did, we should have to say that when a mathematician in a *reductio ad absurdum* proof says "Suppose p ", where p is self-contradictory, he is using the word "suppose" improperly. But no argument can show that this well-established mathematical usage is wrong. If a usage is currently accepted, something must be wrong with arguments which purport to show it is improper. It seems to me that the mistake in arguing that this mathematical use, illustrated in proof A, is improper results from assuming that "suppose" means the same as in argument B. From this assumption it is inferred that "suppose" in context A is improper. But this consequence merely shows the arguments to be wrong, and the proper conclusion about what the arguments show is that the uses are different. That this is the correct conclusion, and that the mistake in the arguments given lies in assuming the opposite I shall try now to make clear.

The gist of the considerations resulting in the paradox that in proof A no supposition is being made is that one cannot consider the possibility of p 's being true unless p is possible nor conceive p 's being true unless p is conceivable, and that one is being asked to do just this in being asked to suppose p , no matter what the context is. Thus "Suppose p " is taken under all circumstances to entail "Consider the possibility of p 's being true", where the latter is understood to involve knowing what it would be like for p to be true. This analysis of the nature of a supposition clearly eliminates the possibility of supposing what is self-contradictory. The question is whether the meaning of "suppose" bears this analysis in all contexts. There is no doubt that in argument B the word "suppose" is so used that in supposing p one is entertaining a possibility. There are a number of contexts where the usage of "suppose" can properly be described in this way. What is supposed is a possible state of affairs, and what is inferred¹ from what is supposed is also

¹ It is to be noted here that the consequence said to be inferred from the supposition does not follow from the supposition, i.e. the connexion between supposition and consequent is not one of logical entailment.

a possible state of affairs, *e.g.* that the pendulum would make the same number of swings everywhere on the earth's surface.

That there is some similarity between the uses of "suppose" in the two arguments A and B is obvious—were it not for the similarity we should not be puzzled about the possibility of making a supposition in A after seemingly having made one. One important respect in which the uses are similar is that in both A and B we say "suppose p " as a preliminary to deriving consequents. In both cases we derive consequents from what we say we are supposing. But in another respect the use of "suppose" in A is very different from that in B: we cannot describe "the supposition" that there is a rational number

$\frac{s}{t} = \sqrt{2}$ as consisting in consideration of the possibility of there

being a rational number $\frac{s}{t} = \sqrt{2}$, for once we do this we are led

to denying that there is any supposition to describe. But what this fact should show us is not that a supposition cannot be made but that the nature of the supposition in such a context as proof A is very different. It is correct English, and it expresses something true, to say that in proof A one supposes there is a rational

number $\frac{s}{t} = \sqrt{2}$. It then remains to be seen what it comes to

to make a supposition of this sort, and how supposing a self-contradiction in the course of a mathematical demonstration differs from what we do in ordinary life in making suppositions. What we do in ordinary life is illustrated by the usage of "suppose" in argument B. This usage is such that to suppose p entails knowing what it would be like for p to be true. Where we do not know what it is like for something to be the case, *e.g.* for a given person to be both alive and dead, we should say it was impossible to make any supposition to this effect. But the mathematical usage in connection with a self-contradiction p is such that to suppose p does *not* entail knowing what it is like for p to be true. When Fermat conjectured that for all integers n , $2^{2^n} + 1$ is prime, it is not the case that he knew what it would be like for this to be true but just did not know how to prove it. He did not know what it was like for all members of this form to be prime, as the disproof of this proposition shows. But he did conjecture, and hence conjecturing in such a case cannot entail what it does in non-mathematical contexts.

If we examine the function of suppositions within the arguments of which they are the first step, and the function of the

arguments with respect to them, we shall have further reason for distinguishing between the usages of "suppose" in arguments A and B. In demonstrations in number theory such as A, in contrast with arguments like B, it is clear from the preceding discussion that certain ways of describing the function of the supposition p are precluded: The supposition is not a preliminary to determining which possibility of p is realized, since p does not have each of two possibilities. If p is possible, its negative will be impossible. Nor can supposing p be described as conceiving something the impossibility of which it is the function of the deduction to disclose: if it is conceived, it is possible. Without doubt the deduction shows that p is impossible, but it could not show that what was conceived is impossible. It looks then as though in making a supposition one has before one, prior to inventing the disproof, only what one utters or writes down.¹ One is unable to *see* anything about its use. For example, one is unable to see prior to disproof whether it does or does not make sense to say every equation of the form $x^2 = a$ has a rational solution. I understand Wittgenstein to have made a similar point in lectures, and I quote from notes the following discussion explicating the contrast between suppositions figuring in mathematics and in ordinary life:

"To make a supposition would normally be to have some kind of picture of the kind of thing that is being supposed. If I suppose that this room is higher than it is, I can say it will be difficult to heat, that it will look so-and-so; and these are consequences which are other than the picture which corresponds to the supposition. But if I say, Suppose there were a proof that so-and-so, I have nothing at all. What comes after that, and what I then say, is all the supposition consists of. I might say there is nothing behind my statement but only something before it, namely, the use made of it. In the case of the room I have one picture which represents it, and other things which are its consequences; but with the proof I have nothing but 'Suppose we have a proof', and what comes after partly describes what my supposition consists in. This shows that what we call a supposition in mathematics is entirely different from what we call a supposition in ordinary life."

I want now to say something about the statement, "What comes after [the supposition] and what I then say, is all the supposition consists of". For this is, I think, intended to make the point that one has no knowledge of the supposition distinct from what the deduction provides,² and thereby to call attention to the function of the demonstration. In argument B we

¹ L. Wittgenstein, lectures at Cambridge University, 1934-35.

² *Ibid.*

conceive a possible state of affairs when we suppose the earth is a perfect sphere, and in saying, "If this were so, a pendulum would make the same number of swings per second everywhere on its surface", we draw a consequence not logically entailed by the supposition, which is another conceivable state of affairs. Clearly in A we are conceiving no state of affairs, nor is the consequence logically entailed by "There is a rational number

$\frac{s}{t} = \sqrt{2}$ ", namely, " t is both even and odd", another conceivable state of affairs. How then are the deduction and its functions to be described? It is important to note that we should never construct an indirect proof on a supposition for which it was obvious to inspection that it was self-contradictory. We should never say anything which was explicitly of the form, "Suppose both p and $\sim p$ ". Were we to do so the deduction would be pointless. The point of such a deduction as A is to show us that our supposition is impossible; it makes us understand, by resulting in the consequence $q \cdot \sim q$, something about our supposition. "Suppose p " is used as the starting point of a process which makes us see something about p . And what we see about p enables us to see that any use of p in a direct proof of the truth of any other proposition is precluded. It also enables us to see that any such expression as "Measure off exactly $\sqrt{2}$ yards of cloth" does not make sense.

Now it might be held that quite often an ordinary inference from an empirical hypothesis is also intended to aid us in determining something about our supposition, namely, whether it is true or false: we derive consequences as a preliminary to seeing whether the hypothesis is or is not in conformity with fact. But it is clearly not the deduction which determines this about the hypothesis, namely, that it conforms or fails to conform with fact, but experience. All the deduction in these cases does is to produce consequences which can be tested. Only experience can show one what is true, or false. By contrast, deduction alone shows what is impossible. Furthermore, arguments such as B, unlike A, are not intended to show anything about the modal properties of the supposition. For both $\sim p$ and p in this case each quite plainly have the same modal property, possibility, and can have only this one.

If I have succeeded in showing that the use of "suppose" in such arguments as A and B is different and that ignoring this fact by taking the usage in B as the paradigm to which any legitimate usage must conform is responsible for arriving at the consequence that no self-contradiction can be supposed, we

are in a position to see what makes Church argue from the fact that a self-contradiction p is supposed that " p " is meaningful. It has been pointed out that the ordinary use of "suppose" is such that supposing p entails knowing what it is like for p to be true. This is to say that the ordinary use of "suppose" is connected with the ordinary use of the word "meaning": " S supposes p " entails " S knows what it is like for p to be true", which entails " p " has meaning. In this sense of "meaning" it is correct to say one could not know what it was like for p to be true if " p " had no meaning. The question is whether the mathematical use of "suppose" is similarly connected with "meaning". Church prejudices the issue in saying that the self-contradictory expression, "for all n , $2^n + 1$ is prime", "has an intelligible meaning for anyone with a knowledge of elementary arithmetic and algebra". For one might know arithmetic and algebra and still deny that it had meaning. I want to hold, not that it has no meaning, but that the mathematical use of "suppose" is such that supposing p does not entail " p " has meaning. And I think that because Church takes the mathematical use of "suppose" to be the same as the ordinary use he argues that supposing or conjecturing p entails " p " has meaning. That is, the same kind of confusion is responsible for this claim as for the claim that nothing self-contradictory can be supposed.

In the sense of "meaning" in which " p " has meaning only if it *could* be known what it is like for p to be true, a self-contradictory expression in mathematics has no more meaning than a Lewis Carroll rhyme. The use of "suppose" and "conjecture" which is connected with expressions having meaning in this sense, i.e. the ordinary use, is such that it is impossible to suppose a mathematical self-contradiction. One could no more suppose that he had five marbles of which he gave away six than suppose a surface was both brown and green all over. For in neither case could one know what it would be like for these propositions to be true. Nevertheless it is obvious that in mathematics it is possible, in *another* sense of "suppose", to suppose what is self-contradictory, and clearly this possibility does not rest on knowing what it is like for what is supposed to be true. Thus in the sense in which " p " has meaning" entails the possibility of knowing what it would be like for p to be true, it cannot be held that conjecturing p in mathematics entails " p " has meaning. Hence when Church urges that in making a conjecture of p it must be the case that " p " has meaning, he can only be right if the sense of "meaning" differs from the ordinary sense just

explained, that is, if there is a sense corresponding to the mathematical sense of "conjecture" which differs correspondingly from that associated with the ordinary sense of "conjecture". This sense must be such as to make the term "meaning" applicable to self-contradictory expressions. For Church's thesis, that self-contradictory expressions in mathematics have meaning, tells us that "meaning" has application to mathematical expressions for self-contradictions (just as "self-contradictions are supposed" tells us that "supposition" is applied to self-contradictions). If Church could point out instances in mathematics in which "meaning" had a commonly accepted application to such expressions, that would be sufficient to establish that there is a different sense of that term. In this event Church could show that Lazerowitz denied meaning to self-contradictory expressions in mathematics only because he had failed to note this non-ordinary use of "meaning".

The question thus arises whether Church can point out in mathematics a common use of "meaning" to describe self-contradictory expressions. Exhibiting such a use would enable him to make his point, that self-contradictory expressions do have meaning. It would be an extremely simple way of doing this and it would provide a conclusive answer to any *argument* brought forward to show that they do not have meaning, just as pointing out a use of "suppose" in connection with mathematical self-contradictions is a simple and completely conclusive way of answering an argument that self-contradictions cannot be supposed. If anyone were to agree with Church's view that what is meaningless cannot figure in a supposition and then go on to deny what Church takes for granted, that self-contradictory expressions can figure in suppositions, it seems plain what his reply would be: "But mathematicians constantly suppose what is self-contradictory. There is no difficulty in doing that." This reply is the most natural one to make. It tells us that it makes sense to apply "supposition" to self-contradictions, and thereby settles any question about the possibility of supposing what is self-contradictory. Similarly, the natural reply to the claim that expressions figuring in suppositions, if self-contradictory, are meaningless, is that "meaning" is constantly applied to suppositions of what is self-contradictory, thereby settling this question by appeal to usage. Pointing out a usage obviates the necessity of arguing for one's view or of meeting, by argumentation, any argument against it. The curious thing is that Church does not employ this simple method of establishing that self-contradictory

expressions have meaning, but resorts to *argument* instead. That he does this is important. For it shows, I think, either that it is not possible to establish his view otherwise, or that he himself does not find it possible to establish it otherwise. I do not believe he resorts to it simply because he *prefers* the method of argumentation to the method of exhibiting cases in which the word "meaning" is applied to self-contradictory expressions. I believe the latter method is not open to him. Whether it is or not, *i.e.* whether the word "meaning" has an established usage in the language of mathematics, Church is in a better position to know than most people. But I should doubt that it did. Here and there one finds the expression "meaningless", *e.g.* "division by zero is meaningless" (whence " $\frac{2}{0} = x$ ", or " $2 = 0$ ", is meaningless). And something like an equivalent of it occurs in *reductio ad absurdum* proofs when it is said of the contradiction deduced, "but this is absurd". These examples would seem to indicate that mathematicians do sometimes characterize self-contradictory expressions as meaningless, and that not all of them would agree with Church that such expressions have meaning "for anyone with a knowledge of elementary arithmetic and algebra". But the fact that disagreement could arise amongst people equally conversant with mathematical usage shows that it is not yet a settled convention to describe self-contradictory expressions as meaningless. And it is not usage, *i.e.* not a settled convention, to describe them as meaningful. Cases of describing them thus, if any exist, are too infrequent to permit of their use by Church to establish his view. Moreover, as just pointed out, there are also cases, even though infrequent, of describing them as meaningless.

Now if the view that self-contradictory expressions have meaning cannot be established by citing common applications of the term "meaning" to them, can an argument (from the fact that self-contradictions are supposed) succeed? Argumentation sometimes is but an alternative way of proving a point. But here an argument is offered because the alternative method of direct substantiation (by appeal to usage) is not available. Church's argument is intended to show that the description "meaningful" is correct. But an argument will show a description to be correct only if there is a *possibility* of citing a commonly accepted application of the descriptive term. So long as one finds it necessary to *argue* that self-contradictory expressions are meaningful because usage of the term "meaningful" as a description cannot be exhibited, the view cannot

be correct.¹ Under these circumstances argument will not show self-contradictory expressions *are* meaningful, although argument might convince one of the appropriateness of so describing them. If mathematicians were to become convinced of this, *i.e.* were to introduce the word "meaning" as a description of any supposition-expression and make it common usage, then Church's argument would establish his view. For then there would be a possibility of citing an accepted application of the term, and argumentation would not be forced upon one as a substitute for direct establishment by appeal to usage. But in advance of its being common usage to describe self-contradictory expressions as meaningful, no argument can show the description to be correct. For no description is correct in advance of its being commonly accepted. Further, even though it were accepted usage to call these expressions meaningful, anyone who did not know this and who thought it was not possible to establish the view that they are meaningful by citing usage, would be wrong in thinking that under such circumstances argument would establish it.

It is clear that if "meaning" has no established use in mathematics as a description of self-contradictory expressions, supposing a self-contradiction will entail nothing whatever about the meaningfulness of the self-contradictory expression. Supposing p will entail that " p " has meaning only if there is a mathematical use of "meaning". What mistake then has Church made? It looks as though he has noted in connexion with ordinary empirical propositions p that "Suppose p " entails that " p " has meaning, that he has taken "suppose" to mean the same in mathematical contexts, and has then gone on to maintain that making a supposition about any mathematical proposition p will entail " p " has meaning. From this it would seem to follow that the sense in which a mathematical expression has meaning will be the same as that in which an ordinary empirical statement has it. For the ordinary sense of "meaning" will be associated with a use of "suppose" which in no way differs from the ordinary use. In claiming for self-contradictory expressions "intelligible meaning" Church seems to lend support to construing "meaning" in this sense. So soon as one sees that in mathematics "suppose" does not have the same use as in ordinary contexts, and that "meaning" has no established use as a description of self-contradictory expressions, it becomes clear that Church's argument is wrong, and that "Suppose p " does *not* entail that " p " has meaning.

¹ By this I do not mean it is *incorrect*. I should say it was neither correct nor incorrect.

IV.—CRITICAL NOTICES.

The Philosophy of G. E. Moore. Edited by PAUL ARTHUR SCHILPP. Volume IV in "The Library of Living Philosophers." Northwestern University, Evanston and Chicago (in Great Britain: Cambridge University Press), 1942. Pp. xvi + 717 pp. 30s.

THE success of the present series of volumes is a function of the character of the critical and expository essays each contains. It is equally unquestionable that the interest and importance of the series depend primarily upon the Replies which the thinkers whose contributions to philosophy the volumes celebrate make to their critics. It is therefore pleasant to report that in both respects—in the generally high quality of most of the essays and in the vigour and seriousness of Prof. Moore's Reply—the present volume continues the high standards set by its predecessors in the series. The essays help to gauge the extent and the kind of influence Mr. Moore has had upon his contemporaries, and his carefully thought out comments upon them exhibit not only his characteristic subtlety and passionate striving for clarity of expression, but also serves as a clear example of his conception of how to do philosophy. It is a matter of profound regret to Mr. Moore's admirers that he has published at such relatively infrequent intervals, so that those who have not had the good fortune to hear him lecture at length or to join in repeated discussion with him have been able to acquire only a fragmentary conception of his mind and personality; and all but members of his current audiences have been compelled to remain in almost complete ignorance of the views he actually holds on issues of great importance. The natural expectation, that most of the best essays in the volume would be by writers who have had some direct contact with Mr. Moore, has certainly been realized. His Reply, together with his charming and revealing autobiographical statement, do help, however, to remedy a long-felt deficiency in the accessibility of his thought. To be the occasion for his writing such a detailed statement of his views on a number of central questions of his philosophy is not the least merit of the present volume.

Mr. Moore is justly regarded as one of the founding-fathers of contemporary philosophical realism; and his career has been often viewed, especially in the U.S.A., as consisting chiefly in defending with extraordinary skill a number of highly specialized "realistic" doctrines in metaphysics, epistemology, and ethics. But the present volume makes it evident that, although he is still to be counted among those who subscribe to theses which may properly be called "realistic," their defence is not, and has not been, his primary concern. Indeed, only a small core of the views he held at the turn of the century, when the realistic polemic against various forms of

idealism began to attract attention, has survived his own critical reflections upon them; and only a small fraction of the essays in the present volume are devoted to those views. The reader of this volume will be impressed by other things than Mr. Moore's realism: his lifelong devotion to making precise the meaning of various questions and statements which philosophers have entertained; his robust faith in the possibility of settling many of the issues between philosophers, if only those issues could be stated with sufficient clarity; his unusual sensitivity to the deceptions which language can play upon even the hardest of thinkers; and his vigorous, clarifying, but unpretentious defence of what he calls "common-sense propositions" against misleading philosophical claims which reject them on the strength of an alleged exclusive and absolute priority of certain epistemological data.

One thing is amply clear: those who seek in philosophy a "complete" or all-embracing world-view will turn away empty-handed from Mr. Moore. In an eminent sense he is a philosophical specialist, a philosopher's philosopher. He has nothing to say to the plain man in the street which would be pertinent to the latter's problems: he has no directives for action and no consolatory rhetoric to offer, and it is difficult to imagine him with a popular following under any kind of social order. Indeed, as he himself notes, neither the world nor the sciences would ever have suggested to him directly any philosophical problems, and it is what other philosophers have said about the world or the sciences which has been the stimulus to his thinking. It is certainly a pity that Mr. Moore has never concerned himself directly with scientific subject-matter, and many of his admirers deeply regret the illumination which they feel sure would have come from his pen had he turned to the vexed issues which beset contemporary philosophers of science. In any event, few of his readers will question the judgment that, seen against the background of the traditional preoccupations of philosophers or of the concerns which arise out of the recent revolutions in science and society, his writings have only a meagre content. However, the absence of a sweeping and comprehensive system in Mr. Moore's writings is a lack which many will find easy to bear. Any number of contemporary thinkers can be found who can produce possibly thrilling but nonetheless imaginary maps of the cosmos, and only a handful who, like Mr. Moore, can preserve resolutely sober and disciplined judgments in the midst of philosophical carnivals. He has remained faithful to his early observation: "To search for 'unity' and 'system,' at the expense of truth, is not, I take it, the proper business of philosophy, however universally it may have been the practice of philosophers". Accordingly, if his readers will never leave the earth on erratic journeys through the uncharted universe when they take him for a guide, neither will they be misled by him into denying what they undoubtedly know about the scenes they inhabit. Miss Stebbing is more optimistic than seems warranted in

declaring, in her essay in the present volume, that anyone who has been able to learn something of Moore's way of thinking could not succumb to the muddle-headed creeds of the current forces of irrationalism. But it is not fanciful to believe that exposure to his mode of thought and analysis is bound to give us a saner and clearer understanding of the things we believe and know.

The nineteen essays in the volume under review fall rather naturally into three groups, one dealing with issues in ethics, one with sense-perception, and one with questions of philosophic method; it is under these headings that Mr. Moore considers them, and that I will discuss them. The two essays in the first group which interested me most are Mr. Stevenson's "Moore's Arguments against Certain Forms of Ethical Naturalism" and Mr. Frankena's "Obligation and Value in the Ethics of G. E. Moore." Mr. Stevenson maintains that Moore has not refuted the view according to which "*x* is right" has the same meaning as "I approve of *x*". He considers the three arguments Moore used against this view: that on the proposed definition something may be both right and wrong (e.g., right for *A* but wrong for *B*); that something which is *now* right may formerly have been wrong; and that when *A* says "*x* is right" while *B* says "*x* is wrong" they are *not* differing in opinion, though they would normally be understood as doing so. Mr. Stevenson argues that Moore's first conclusion is obtained only by neglecting to note that the reference of the pronoun "I" is to the *user* of the sentence "I approve of *x*"; that the second conclusion can be avoided by amending the proposed definition so as to restrict the reference of "right" and "wrong" to the attitude of the one using them *at the time* that he does use them; and that the third conclusion is obtained only on the supposition that a "difference in opinion" must be something entirely "cognitive", whereas, in fact, the phrase in the present context should be understood as connoting a disagreement in *attitude*. And *a propos* of this last point Mr. Stevenson notes that the proposed definition requires emendation, in order to bring out the "emotive meaning" of expressions like "*x* is right"; but he does not state explicitly the form of the revised definition. Mr. Moore admits that his first argument is indeed fallacious. He notes, however, that although the definiendum and the definiens in the proposed definition are said by Mr. Stevenson to have the same "cognitive meaning", they cannot in fact have the "same sense": for on Mr. Stevenson's own admission the definiendum has a stronger "emotive meaning" than the definiens. Moreover, he recognizes the possibility that in asserting the rightness of *x*, *A* may not be asserting anything which could possibly be true or false and is only issuing a *command*; accordingly, typically ethical words like "right", "wrong", and "good" may not, after all, be names of *properties* of things and actions. On the other hand, he notes that Mr. Stevenson has not shown either that the latter's "naturalistic ethics" is true or that the views expressed in *Principia Ethica* are

false on the point at issue. Nevertheless, Mr. Moore admits that he has "some inclination" to think Mr. Stevenson's view is true, and also "some inclination" to think his own earlier views are true: "If you ask me to which of these incompatible views I have the stronger inclination, I can only answer that I simply do not know whether I am any more strongly inclined to take the one than to take the other". This is surely a handsome confession of Mr. Moore's doubts, and indicates that the case for a naturalistic ethics is not a hopeless one in his eyes. Those who are convinced that Mr. Moore's earlier views are irrelevant, if not completely wrong, to most of the moral problems which agitate men in their public affairs, are entitled to the legitimate hope of winning Mr. Moore completely over to their side of the discussion by suitably sharpening their arguments and making more precise their distinctions.

Mr. Frankena's essay tries to show that in terms of Mr. Moore's conceptions of the right and the good there is no necessary connexion between obligation and intrinsic value. If goodness is a simple, non-natural property, so Mr. Frankena argues, " x is intrinsically good" cannot be synonymous with " x ought to exist for its own sake" as Moore once implied; for the latter phrase refers to a notion which has a complexity that the notion of goodness lacks. Moreover, Mr. Frankena insists that goodness can have a normative character only if it *analytically* involves a reference to an agent on whom something is enjoined; in other words, goodness can be normative only if it is not a simple intrinsic property. Moore's ethics, in which duty is defined in terms of goodness, has therefore no place for the notion of obligation. If, however, goodness is not normative, Mr. Frankena believes that there are no good reasons for supposing it to be indefinable or non-natural: for according to him it is the apparently *normative* character of ethical judgments which makes them irreducible to judgments about a "natural" subject-matter and which deprives them of any "descriptive" force. Mr. Moore devotes his longest comment to these attacks on his position. He admits at the outset that it was a mistake on his part to imply that moral obligation could be defined in terms of goodness. But he vigorously denies that the non-normative character of goodness follows from his admission that "Statements of the form ' x is good' neither include nor are identical with any statement about obligation". The grounds for his denial may be stated as follows: According to Mr. Moore, one statement may *follow from* another even though the relation *follow from* depends upon a "synthetic" rather than an "analytic" connexion; for example, using an illustration taken from Mr. Langford's essay, Mr. Moore holds that the function *x has twelve edges* follows from the function *x is a cube*, although the latter function "does not include nor is identical with" the former one. Accordingly, he thinks that even if goodness is a simple or an intrinsic property, the fact that it is normative, as well as other facts about obligation, *may* follow from it, though of course

not analytically. I cannot here summarize the remainder of Mr. Moore's rebuttal; but I should like to comment briefly upon the part of the rebuttal cited. The force of Mr. Moore's rebuttal clearly depends on the correctness of his view that one statement may follow from another without being analytically contained in the latter. Moreover, it should be noted that even if this supposition is admitted as valid, Mr. Moore does *not* show that statements about obligation do *in fact* follow (in the special sense indicated) from statements about intrinsic value. But is the supposition valid? Mr. Moore relies heavily, and as far as I can determine entirely, upon the illustration he has borrowed from Mr. Langford; and I must submit that the illustration is not conclusive. I am sure that mathematicians skilled in the foundations of geometry are prepared to show in detail that the connexion between the functions x is a cube and x has twelve edges, in virtue of which the second follows from the first, is entirely an *analytic* one. To be sure, it is possible to know that an object is a cube without knowing (except perhaps in a dispositional sense) that it has twelve edges, just as it is possible to know that 43 is an odd number without knowing it is a prime. But to admit this is simply to admit—so it seems to me—that one may know something without knowing everything logically contained in what one knows; and I can find no good reason for converting what is an unquestionable fact of human psychology into an occasion for invoking a non-analytic (and obscure) notion of *following from*. However this may be, Mr. Moore does not seem to me to have established his case that Mr. Frankena is wrong on the main points at issue between them.

Two other papers in this group merit some attention. Mr. Edel's long essay, "The Logical Structure of Moore's Ethical Theory", is an ambitious attempt to specify "the terms employed in the theory, the definitions and postulates governing their use, the rules for the formation of ethical statements, and the procedures for interpreting ethical terms and determining the truth or falsity of ethical statements". In addition, he aims to show that partly as a consequence of this "logical structure" Mr. Moore is more or less committed to a distinctive set of specific values. Mr. Edel's attempt is an interesting one, though not altogether successful. Part of the difficulty lies in the fact, as Mr. Moore rightly notes, that Mr. Edel frequently obscures his intent by the language he employs. For example, two of the "postulates" cited which are said to govern "the construction of ethical statements and the use of ethical terms" in Moore's ethics are the following: "The intrinsic value of x depends solely on the intrinsic nature of x ", and "Good is a simple or unqualified predicate". But surely these statements, when they occur in Mr. Moore's formulations, occur as *conclusions* of his analyses, and never as "mandates issued by Moore for the construction of ethical statements". Again, Mr. Edel maintains that since the postulates contain the two fundamental *undefined* terms, "intrinsic property"

and "intrinsic value", they require an "explicit interpretation". It is, however, far from evident why these terms need an "interpretation" simply because they are undefined; for Mr. Moore has never used them without attaching a meaning to them, and they do not function in his system as *variables*. The trouble seems to be that Mr. Edel construes Moore's ethical theory as if it were an abstract postulate system quite analogous to the formalized and only partially interpreted postulate systems for a "geometry". And this seems to me an error, just as it would be an error to regard any group of *propositions* (for example, propositions about human heredity) as if they were an abstract set of uninterpreted *sentential forms*—though of course it is possible to construct for any set of propositions an appropriate system of *sentential functions* such that the propositions will be *values* of these functions. Mr. Edel's discussion is therefore highly suggestive without being adequately clear. He also makes the further point that the logical structure of Moore's theory tends to lead inquiries into ethical issues *away* from concrete empirical goods and *towards* a contemplative beholding of values; for according to him Moore's ethics is an ethics of vision, not of action. This line of speculation is unquestionably interesting and has often been fruitful of much insight. If I am not convinced by Mr. Edel's discussion of these matters it is not because I think the sort of questions he raises are illegitimate, nor because he offers no evidence whatsoever for his conclusions. I am unconvinced because the analysis of philosophical theories in terms of their sociological causes and consequences is a most difficult and complex undertaking, and because Mr. Edel's evidence for his conclusions is too fragmentary to warrant them.

Mr. Broad's essay, "Certain Features in Moore's Ethical Doctrines", argues two separate points. It first tries to show that Mr. Moore was mistaken in his proof of the self-contradictory nature of Ethical Egoism (the doctrine according to which each man has a predominant obligation to himself *as such*, and an extreme form of which maintains that a man's ultimate obligation is *only* to himself). Mr. Broad claims that although this doctrine is incompatible with Ethical Neutralism (the view according to which the fundamental duty of each of us is to maximize as far as lies within our power the balance of good over bad experiences throughout the world), it does not *contradict itself*. To this point Mr. Moore replies that the extreme form of Ethical Egoism implies an assertion *A* which is self-contradictory. Assertion *A* says, among other things, that the function γ : *x* knows that choice *y* would procure for himself a 'more favourable balance' of intrinsically good over intrinsically bad experiences than any other he could make, and knows also that choice *y* would be at least as favourable to the development of his own nature and dispositions as any other he could make entails the function α : *it would not be wrong for x to choose y*. But according to Mr. Moore, Ethical Neutralism implies an assertion *B*. Assertion *B* says that

the function not- β : *x* knows that the world would be intrinsically worse if he chose *y*, than if he made another choice open to him entails the function not- α : it would be wrong for *x* to choose *y*. Mr. Moore then argues that *A* not only asserts that γ entails α , but implies (even if it does not assert) that γ does not entail β . However, since α entails β follows directly from *B*, if *B* is true then it is self-contradictory to assert that γ entails α but not β . Mr. Moore's argument leaves me puzzled. In the first place, the contradiction is exhibited on the hypothesis that *B* is true, so that on the face of it what has been shown is the incompatibility of *A* with *B*, and not the self-contradictory nature of *A*. And in the second place, if his argument is good enough to show the self-contradictory nature of *A*, why is it not equally good for showing the self-contradictory nature of *B*? For *B* asserts that not- β entails not- α , and it seems plausible to hold that in addition it implies that not- β does not entail not- γ ; however, since not- α entails not- γ follows directly from *A*, if *A* is true then it is self-contradictory to assert that not- β entails not- α but does not entail not- γ .

Mr. Broad's second point deals with the distinction between natural and non-natural properties, and he argues that the criterion Moore offered for the former in his *Principia Ethica* reduces all characteristics (even such traits as brownness and roundness, which are held to be natural ones) to non-natural ones. Mr. Moore promptly disavows his former suggestion for a criterion of natural properties as "utterly silly and preposterous". He now thinks that an intrinsic property ascribed to a thing is natural when by that ascription the thing is "described" to some extent, and that such a property is non-natural when the thing is thereby not "described" at all. However, he declares his inability to specify the sense of "description" which is in question. But is it not possible to arrive at the required distinction in somewhat the following manner? Consider first, all the primitive terms of *physics* which for one reason or another may be taken to express intrinsic properties, as well as those definable exclusively by their means: these will be the "descriptive" terms of physics; then consider the primitive terms (if any) of *chemistry* which express intrinsic properties, as well as those defined by their means or by means of the descriptive terms of physics: these will be the "descriptive" terms of chemistry; perform an analogous grouping of terms for *biology*, *psychology*, and the other "positive" sciences; the logical sum of these different classes of terms will be the "descriptive" terms which express "natural" properties, while terms not belonging to this class will express "non-natural" characteristics. To be sure, such a procedure does not provide an easy criterion for determining whether a given property is a natural one or not, and it has what from Mr. Moore's point of view may be a drawback in that it relativizes the distinction to a given state of the natural sciences. But the procedure does seem to me to have the advantage of specifying a

distinction which is closely related to the conduct of scientific inquiry ; and it supplies a method in terms of which it becomes possible, at least in principle, to determine unambiguously whether a given property is natural or not.

Of the five papers devoted to problems connected with sense-perception, Mr. Bouwsma's "Moore's Theory of Sense-Data" seems to me by far the most interesting and ingenious. He shows very effectively that the directions Moore gives in his "A Defence of Common Sense" for identifying (or "picking out") sense-data are quite unsatisfactory, since those directions do not enable one to find anything satisfying Moore's descriptions. Mr. Bouwsma therefore raises a doubt as to whether there are such things as visual or tactile sense-data which are distinguishable from the surfaces of physical objects. However, he suggests possible reasons which might mistakenly lead one to suppose there are such things : namely, certain analogies between hearing, tasting, and smelling on the one hand, and seeing and touching on the other. Thus, when we hear a rat we may say quite correctly that we hear a gnawing sound which is distinct from the rat we hear ; and we may therefore be persuaded to think that there must be a visual something which we see when we see a rat, and which is distinct from the rat we see. Accordingly, since there are auditory sense-data (that is, there are *sounds*), we come to believe there are visual sense-data distinguishable from the surfaces of bodies ; but Mr. Bouwsma thinks, however, that we thereby "make up for the deficiency in the facts from which we start by inventing a new vocabulary". Mr. Moore readily admits that the directions he gave for "picking out" sense-data were not sufficiently clear, and he therefore supplements them in a manner more satisfactory to himself—in terms of the distinction between *seeing an object* (in the sense in which we may see a part of the surface of a table) and *directly seeing an object* (in the sense in which we may see an after-image with our eyes shut). Accordingly, he claims that the proposition expressed by "The object which I *see* is part of the surface of my hand" is not identical with the proposition expressed by "The object which I *directly see* is identical with part of the surface of my hand". In fact, he claims to know that the first proposition is true, but finds the second one doubtful. On the other hand, he admits that although he has this doubt, he also knows that the object he is seeing which is part of the surface of his hand is part of that surface ; consequently, if what he is directly seeing is identical with part of the surface of his hand, he must be feeling sure of and doubting the same proposition. How to resolve this paradox he does not pretend to know ; and since he thinks that the distinction between *see* and *directly see* can be carried over to the other modes of sense-perception, so that, for example, he finds it doubtful whether any physical sound we *hear* is *directly heard*, analogous paradoxes can be constructed for them. Mr. Moore makes it clear, however, that he is using "sense-data" in

such a way that it is *not* a contradiction to say of an object that it is *both* a physical reality *and* a sense-datum. He therefore finds that the issue between himself and Mr. Bouwsma is that while he thinks there are some good reasons for denying that what we directly perceive is a physical reality, the latter thinks there are none. The issue is not one to be settled in an off-hand manner, certainly not in a review devoted to many other questions. I will merely state that Mr. Moore's new set of directions for "picking out" visual sense-data—e.g., in such cases as when I am seeing my hand—still do not satisfy me. I find that I do see after-images under certain circumstances, and I think therefore that I understand how Mr. Moore is using "directly see". But I do not find anything analogous to after-images when I am seeing my hand, and I cannot therefore persuade myself that what I directly see when I am seeing my hand is possibly not identical with a part of the surface of my hand. I also suspect, though I cannot establish this, that some of Mr. Moore's doubts are consequent to his somewhat peculiar conception of what is a *physical reality*; in any event, I must confess my own doubts whether his use of this phrase is in accordance with either the best or common usage.

Mr. Marhenke's "Moore's Analysis of Sense-Perception" discusses some of the grounds from which the non-identity of visual sense-data and parts of physical surfaces is usually inferred. One premiss contained in those grounds is that if a sense-datum *appears* to have a quality it really has that quality; and though Moore has been able to find no conclusive reasons for its truth, Mr. Marhenke thinks there are no conclusive reasons for challenging it. Another such premiss, concerning which he does have reservations, is that physical objects possess certain *real* spatial properties and stand to each other in certain *real* spatial relations. Mr. Marhenke argues that only the *topological* spatial characteristics of objects are intrinsic to the latter. Furthermore, and this is the important point in the discussion, he attempts to show that topological traits are the only spatial traits intrinsic to *sense-data* as well: he introduces considerations, drawn from the relativity of metrical properties to the standards of measurement employed, to prove that shapes and sizes are *extrinsic* properties of sense-data and not only of physical objects. He therefore believes that he has vindicated the possibility that sense-data can be identical with parts of physical surfaces. It is this part of Mr. Marhenke's argument that Mr. Moore questions, and it seems to me rightly so: it is "fantastically untrue", according to him, that "independently of a standard of comparison it is impossible to compare two different sense-data in respect of length or size". Nonetheless, he admits he is "strongly inclined" to hold with Mr. Marhenke, as well as with Mr. Murphy's essay, "Moore's 'Defence of Common Sense'", that we do directly see parts of physical surfaces, even though he believes—as already noted—that there are good reasons for doubting this.

Mr. Ducasse's paper, "Moore's Refutation of Idealism", argues, in opposition to Moore's well-known essay, that there is a class of cases for which it is true that the *esse* of things is *percipi*; but it also maintains that for another class of things this is not so. The argument is constructed around the distinction between the *connate accusative* of an act (as in jumping a *jump*, jumping a *leap*, or waving a *farewell*) and the *alien accusative* of an act (as in jumping an *obstacle*, jumping a *ditch*, or waving a *flag*). Mr. Ducasse maintains that the connate accusative of an act can exist only in the occurrence of that act; accordingly, if we call the accusative of any cognitive act its "cognitum", any cognitum connate with its activity will exist only when the latter exists. Mr. Ducasse holds such cognita as *blue* or *bitter* to be connate accusatives of the experience called "sensing"; and he thinks that "blue" and "bitter" are not names of *objects* experienced nor of *species* of such objects, but are names for *species of experiencing*. As he puts it, "to sense blue is to sense *blueily*"; accordingly, in any case of an awareness of blue, what one is aware of is said to be the determinate nature of one's awareness. Since Mr. Moore abandoned the position he took in the paper under discussion some time ago, it is not surprising to find him in agreement with Mr. Ducasse's general conclusion. However, he draws attention to the fact that words like "blue" and "bitter" are used in two senses, and that Mr. Ducasse has not distinguished them: the sense in which we say that quinine is bitter, and the sense in which we say that what we directly taste is a bitter taste. He therefore finds it nonsense to hold that bitter, as a property of quinine, is a species of the tastes we taste—for bitter as a property of quinine or other physical objects may exist without being perceived. But conversely, Mr. Moore also thinks that in saying a taste we are tasting is bitter, we are *not* saying anything at all about the property of quinine which we attribute to it in declaring it is bitter: in saying of quinine that it is bitter, we are really saying that the quality attributed to a taste (when the latter is declared to be bitter) "is related in a certain way to quinine, though not, of course, that it is a quality of quinine, nor yet the absurdity that it is a property . . . of quinine". Mr. Moore therefore defines the issue between himself and Mr. Ducasse as follows: he once held that bitter (in the second sense) could exist without being perceived, while Mr. Ducasse thinks it could not; and he now also thinks that it could not, for he is "strongly inclined" to hold that nothing directly perceived can be identical with any part of a physical object. On the other hand, he does not believe that Mr. Ducasse has proved his point: for Mr. Ducasse has not shown that the directly perceived quality *blue*, for example, is related to one's seeing of it in the same way as the particular kind of dancing one may be engaged in (e.g., waltzing) is related to one's activity of dancing. With this final observation I find myself in complete agreement.

Mr. Malcolm's "Moore and Ordinary Language" seems to me

the best and most rewarding in the final group of papers, if not in the whole volume. It exhibits a penetrating grasp of Mr. Moore's method of philosophizing, and gives a most persuasive and illuminating account of the rationale behind his defence of "common-sense". It shows that the sort of refutation Mr. Moore supplies to typically philosophic pronouncements (e.g., "There are no material bodies") is not question-begging as it is frequently held to be; and it makes evident that the essence of his refutations consist in pointing out that such pronouncements "go against ordinary language". Mr. Moore's defence of common-sense propositions thus consists in calling attention to the fact that philosophers are using language in extraordinary ways: the philosophers are themselves not always aware that the meanings they attach to the phrases they use have frequently only the faintest analogy to the meanings those phrases have in the contexts which actually determine their use. Many philosophical puzzles may consequently be resolved by bringing to the surface the point that even though there are such analogies, there are also profound *dissimilarities* between the way philosophers use certain expressions and the way they are ordinarily used. Accordingly, if the philosophical assertions in question are taken in the ordinary sense of their constituent phrases, it becomes easy to see that the assertions are false or nonsensical; and since philosophers rarely specify any other sense for the language they use than the ordinary sense, their assertions have the appearance of wilful paradoxes. Mr. Malcolm seems to me right in noting that Mr. Moore does not consistently bring out the linguistic, non-empirical nature of philosophical paradoxes; and he seems to me no less just in his observation that Mr. Moore does not probe to the sources out of which so much philosophical confusion stems. Students of Mr. Moore's philosophy will be grateful to Mr. Malcolm for his unusually capable analytical version of Mr. Moore's procedure.

Mr. Murphy's essay already cited also touches upon some of the points developed by Mr. Malcolm in other ways. It emphasizes the service Moore's defence of common-sense has performed in demanding recognition for the fact that "the understanding which is sufficient for common-sense purposes" is *not* "philosophically of a makeshift and derivative sort", as well as for the further truth that the kind of understanding and knowledge involved in the common-sense view of the world "are precisely the sort which a philosophically comprehensive estimate of the world we live in and our ways of finding out about it should have led us to expect in this situation". In her essay, "Moore's Influence", Miss Stebbing also emphasizes the point that sense-data possess no absolute epistemological priority, and maintains (in opposition to her earlier views) that sense-data are *isolable* but not *isolated* entities, whose discrimination proceeds in "common-sense" contexts and is effected for specific purposes arising within such contexts. It is gratifying to have such a forthright statement from her on this

matter. Of no less interest is her judgment that Moore's lasting influence is to be found in the "same level analysis" of common-sense propositions he has stimulated—"in the analytic definition of expressions and in the analytic clarification of concepts"—rather than in his "directional" or "new level analysis" of them.

Two papers, "Moore's Paradox" by Mr. Lazerowitz, and "Moore's Proof of an External World" by Miss Ambrose, develop what seems to me essentially the same point. The paradox which Mr. Lazerowitz notes is that philosophers *know* facts inconsistent with the creeds they profess: for example, they *know* that some events happen before others, though they may assert that time is unreal; and he concludes that therefore Mr. Moore's "refutations" of such creeds by calling attention to the facts philosophers undoubtedly know are not refutations at all. He first argues that the philosophical propositions in question are not empirical, and cannot be settled by appealing to empirical data; he also tries to show that those propositions are not necessary truths either, though they are nevertheless incapable of refutation. He therefore maintains that such propositions are in effect *proposals* as to the way ordinary expressions should be used; and he also holds that Moore's "refutations" are simply *counter-proposals* which recommend that the ordinary usage of common language be not altered. Miss Ambrose likewise thinks that the sceptic who doubts the existence of an external world is merely offering a disguised proposal, according to which statements like "No one *knows* that hands exist" are to be accepted as necessary truths. She believes that Mr. Moore has not seen clearly what the sceptic is really doing, and has not shown how pointless the sceptic's proposal really is: for if the proposal were adopted with respect to the use of the word "know", a new term would presently have to be introduced (or an old term be given a new use) in order to perform the function that "know" now has. She, too, thinks that Mr. Moore's "proof" of an external world consists of his urging the preservation of the linguistic *status quo*. Both papers seem to me suggestive, but I agree with Mr. Moore that neither of them offers any respectable argument to show that the philosophical statements discussed really are disguised proposals. Moreover, neither paper pays any attention to the point that the sort of philosophical views each examines have arisen historically as the outcome of reflection upon *empirical data*; and though, as I believe, the evidential force of those data has been frequently misconceived, I do not find the thesis plausible that the mistakes which have occurred in estimating this evidence are simply failures to distinguish between genuine statements and recommendations for linguistic innovations.

Mr. Langford's provocative essay, "Moore's Notion of Analysis", contains, among a number of other things, a formulation of two different views as to the nature of *analysis*, and a challenge to Mr. Moore to indicate to which of them he subscribes. According to

the first (or "conceptual") view, the analysandum and the analysans are both *ideas* or *concepts*, where the latter is required to be "more articulate" or "less idiomatic" than the former, and must therefore be a "grammatical function" of more than one idea. Consequently, anyone using the verbal expression for the analysans will mention the same objects as are mentioned by the expression for the analysandum, but will do so by reference to *other* objects. Mr. Langford therefore believes that though the analysandum and the analysans are in some sense "cognitively equivalent", the verbal expressions for them cannot be synonymous. For example, he holds that on the present view, "*x* is a cube if and only if *x* is a cube with twelve edges" expresses an analysis of the notion of being a cube; but "*x* is a cube" is not synonymous with "*x* is a cube with twelve edges", since, according to him, having twelve edges is not part of the notion of being a cube, and is not even *analytically* contained in the latter. On the second ("formal" or "logical") view of analysis, the analysandum and the analysans are verbal expressions, in which the analysans expresses more adequately what is meant by the analysandum. On this view these two expressions will be synonymous. What saves analysis in this sense from triviality is the fact that the analysans reveals the meaning more discursively, more clearly, and more precisely, than does the analysandum. Mr. Langford thinks that if this is the sense of analysis appropriate to Moore's distinction between knowing a statement to be true and knowing an analysis of that statement, then it is difficult to suppose there are any ideas peculiar to philosophy or to logic. Mr. Moore, in his reply, makes it clear that he never intended to use "analysis" in the second of the above senses, and that for him the analysandum is never a verbal expression, and is always a concept or proposition. Moreover, he finds that the first sense does not correspond to his intended usage either: he denies that "*x* is a cube if and only if *x* is a cube with twelve edges" expresses an analysis of the concept *being a cube*; for although, according to him, this concept is *logically* equivalent to the concept *being a cube with twelve edges*, the alleged analysis does not satisfy each of three conditions he regards as necessary for something to be an analysis in his sense. These three conditions are: no one can be in the position to *know* that the analysandum *applies* to an object without knowing that the analysans also applies; no one can be in the position to *verify* that the analysandum *applies* to an object without verifying that the analysans also applies; any expression expressing the analysandum must be *synonymous* with any expression expressing the analysans. Thus, the statement, "The concept *being a brother* is identical with the concept *being a male sibling*", gives an analysis of the first concept mentioned which satisfies these conditions. On the other hand, Mr. Moore does not know how to resolve the paradox posed by Mr. Langford: if the verbal expressions representing the analysandum and the analysans have the same meaning, the analysis appears to

be trivial and to state "a bare identity"; and if they do not have the same meaning the analysis must be false. He feels sure that the statement of the analysis of *being a brother* just cited does *not* assert the identity which is stated by "To be a brother is to be a brother"; but he is not sure *why* it does not. He suspects that the statement of an analysis must, in some sense, be about the *expressions* used for the analysandum and the analysans, as well as about these concepts; and the only further suggestions he can make is that the expression for one concept must not only be different from the expression for the other, but that the expression for the analysans must *explicitly mention* concepts not explicitly mentioned by the expression for the analysandum. What is one to say to all this? One thing surely: if this is the way Mr. Moore intends to use "analysis", this is the way he intends to use it. But *cui bono*? It is certainly not evident for what ends analysis in this sense is important. In the light of Mr. Moore's third necessary condition for his sense of "analysis", it seems clear that whether an analysis of a concept is possible depends very largely upon the accidents of the language one is using. Is the concept *goodness*, for example, analysable? Not if every expression for it (presumably in the English language) is not *synonymous* with any expression which may be used for the alleged analysans! Is it surprising that *goodness* is found to be unanalysable by Mr. Moore? Moreover, many expressions for concepts are highly vague, and it is most unlikely that more complex expressions can be found which are synonymous with the first in a given language. It is undeniable, nevertheless, that those concepts can be clarified in important ways, and that Mr. Moore has often given us a clarified view of some of them, in spite of the absence of synonyms for them. I am curious to know why Mr. Moore thinks analysis in his sense is peculiarly important; and I think I am expressing more than an idiosyncratic desire in wishing that he would soon explain what special sort of clarification analysis in his sense can achieve.

Mr. McGill's "Some Queries Concerning Moore's Method" is a *mélange* of unbelievably crude and irrelevant criticisms of Moore's method and views. Mr. McGill finds that the distinction between melioristic philosophies (those really devoted to transforming the world into a better place) and non-melioristic ones is perhaps the most "important" distinction between philosophies. He also finds that the problems with which Moore has been concerned are narrowly abstract and socially unimportant—though he presently qualifies this latter judgment by admitting that if "materialism" in philosophy tends to promote the adoption of an efficient social organization, then Moore's "realism" is a valuable factor in "the present struggle against fascism". Mr. McGill finds fault with Moore's critique of William James' theory of truth on the ground that while Moore's method of refutation is formally correct, it overlooks the essential point that James' views were aimed at bringing about important changes in human society. He also declares that the

fact that pragmatism continues to dominate the philosophic scene "seems to suggest that Moore's disproof is decisive only against certain of James' formulations". If Mr. McGill means what he says, it follows that he must regard it as pointless to try to exhibit the inadequate bases for all beliefs which continue to dominate large sectors of the human scene, including beliefs which he himself must regard as false, for example, spiritualism and other forms of idealism and non-materialistic philosophies. It is depressing, to say the least, to find in a professional philosopher such a ready acceptance of the primacy of sociological analysis and such a sentimental criterion for the adequacy of a philosophy. As for Mr. McGill's dicta on the law of excluded middle (which he thinks "breaks down" when we deal with "transitions") or on the issues at stake in the doctrine of the internality of relations (he holds that whether a relation is internal or not depends upon the social and scientific "utility" of certain classifications), I can only say that they seem to me a hopeless muddle.

No mention has been made thus far of several essays in this volume. I have not discussed them in their proper place either because I have been unable to make sufficiently clear sense out of them, or because I have found them only moderately interesting. The first reason covers Mr. Wisdom's "Moore's Technique" and Mr. McKeon's "Propositions and Perceptions in the World of G. E. Moore". Mr. Wisdom's essay is written in his now well-known meandering style, in which nearly everything is said by indirection. It requires more courage than I possess to venture even a synopsis of his paper, to say nothing of an opinion about it. I do not understand why a responsible and competent philosopher finds it necessary to place such a burden upon his readers. Mr. McKeon's essay contains a severe criticism of Moore for his preoccupation with perceptions and propositions rather than with the objects of our cognitions, and for his method of interpreting other philosophers' doctrines. Mr. Moore's shortcomings in the first respect are attributed to what is called a "double dislocation in Mr. Moore's World": a dislocation from things to perceptions, according to which Moore is said to demonstrate "the *existence* of things from examination of knowledge" instead of constructing "*knowledge* about things . . . from examination of things"; and a dislocation from cognitions to assertions, according to which Moore is said to demonstrate "the *existence* of things from common *beliefs* about perceptions" instead of analysing propositions so as to clarify *true* cognitions. Mr. McKeon also finds that in lieu of tracing "the causal connexions of the material things of Mr. Moore's real world" or the logical connexions of his propositions, "Mr. Moore has, in effect, reduced logical necessity to a duplication—or a kind of inversely oriented parallel—of causal necessity". When taken at their face value, these cryptic pronouncements are preposterously false, and are clearly belied by what Mr. Moore does do. *Where* does Mr. Moore demonstrate the existence of things from an examination of knowledge, *where* does he offer "common

usage" as the reason for the propositions he believes, *where* in his writings is logical necessity reduced to a "duplication" or a "parallel" of causal necessity? I confess to some sympathy with the general sense of Mr. McKeon's complaint that Mr. Moore's materials of analysis are often very "thin" indeed. But I cannot understand by what possible line of reasoning Mr. McKeon arrives at his view that the inadequacies of Mr. Moore's philosophy arise from the fact that he hasn't been doing the sort of things either Plato or Aristotle are alleged to have done. Why should Mr. Moore have tried to do either? Nor is it clear to me how Mr. McKeon can be right in maintaining *both* that Mr. Moore's method of analysis "determine[s] the truths he chooses to analyse", *and also* that "his philosophic method is adapted primarily to treat of perceptions and propositions"; for if Mr. Moore *adapted* his method to certain materials, that method could not have determined the *choice* of those materials. As for Mr. McKeon's charge that Mr. Moore's method of construing the statements of other philosophers is a dubious one, it is worth noting that he supports his claim Mr. Moore has misconstrued those statements only by his flat assertion that this is so. But if Mr. McKeon obtains his own construction of what those other philosophers intended by their statements with the help of a method of interpreting philosophical texts he illustrates in his present essay, I can see no warrant for accepting his charge against Mr. Moore.

The other hitherto unmentioned papers are: "The Alleged Independence of Goodness" by Mr. Paton, "Freedom and Responsibility in Moore's Ethics" by Mr. Garnett, and "On How We Know That Material Things Exist" by Mr. Mace.

ERNEST NAGEL.

The Nature of Thought. By BRAND BLANSHARD. London: George Allen & Unwin, Ltd., 1939. 2 Vols. Pp. 654, 532. 32s.

IN order to remove misapprehensions I had better start by disclaiming any personal responsibility for the long delay in producing a review of this book. I was at any rate only asked to review the work in August, 1943. In view of the great merits of the book the delay is particularly unfortunate, and I could only wish that the quality of the review would make up for its long absence, and match the profundity and lucidity of the work reviewed. The appearance of the work is particularly desirable because of the fact that it represents a form of thought which is undergoing a period of eclipse and is rarely now exemplified or explicitly defended, but which yet is extremely important both intrinsically and historically. It is a generation since any such sympathetic large-scale defence of what, for want of a better name, I shall call idealist epistemology

has been published, and a very important gap has been filled by the author's reply to modern criticisms. Not that the work is just a polemic in reply to certain specific points of objection, it is a full-scale survey of the nature of thought and much of it might well be accepted by all schools. In the course of the review I shall dwell mainly on the more controversial points because I have more to say about these, but the reader must not think they exhaust the work. A large part of it is occupied by a very interesting psychological survey of the nature of thought which would be harder to attack and should be of great interest to philosophically inclined psychologists as well as philosophers. But I shall say very little about this side of the work because I have no major criticisms to bring, and it is impossible to give a summary that could do it anything like justice. The original aim of the book was indeed psychological, being to examine the interactions between intelligence and the other functions of human nature, but as it proceeds the interest in metaphysics becomes quite dominant. The author thinks that this is due in part to a certain advance in insight and he therefore attaches a somewhat higher value to the later part of the book than to the earlier (p. 14), but the psychological part is not to be regarded as irrelevant to the other or *vice versa*. On the contrary, one of the author's main aims is to build a bridge across the chasm which separates those metaphysicians who treat thought in complete abstraction from its psychological setting from those psychologists who treat thought in such a way as to make it incomprehensible that it could ever lead to knowledge of anything (p. 13).

The great dimensions of the book render it inevitable that much should be omitted in a review, and for reasons which I have given I shall say next to nothing about Book I, which is mainly philosophical psychology, except that it is in my opinion an excellent piece of work. Points which deserve special attention from the philosopher are Prof. Blanshard's account of "implicit inference" (p. 86 ff.), the exposition of the idealist view of the "given" (p. 118 ff.), and the account of the sub-conscious (p. 173 ff.). I do not think the discussion of philosophical theories of perception comes quite up to the level of the rest of the work. In Book II he turns from perception to the idea. His criticisms here of Russell's view in *Analysis of Mind* and of the Behaviourists are particularly good. But the author agrees with them in refusing to admit the existence of mental acts, as not introspectively discoverable. The meaning and philosophical implications of this denial are, however, not made adequately clear. It is claimed that the usual arguments for their existence, e.g., that the mind is not itself blue when it sees blue, may be met by talking of events of appearing instead of acts (pp. 406-408), but it is not clear what exactly the difference is. An event of appearing must surely be an appearing to *consciousness*, and does not this bring us back immediately to mental acts? He

insists that what we call differences in cognitive acts are differences in content, but I think he can only make his denial of mental acts appear plausible by putting into the content more than it will stand. The kind of thing I mean is this: we agree that there is something present in thought over and above images (few have argued as impressively to establish this as Prof. Blanshard himself), but while Prof. Blanshard denies that mental acts are introspectively discoverable I should be inclined to deny that ideas are discoverable, as something additional to images, in the *content* of the mind. I do not deny any more than Prof. Blanshard that there are elements in objective reality which are intrinsically incapable of being represented adequately by imagery and of which we yet are conscious, but what I seem to find when I consider my mental state, as opposed to my object of thought, which is by no means necessarily part of my mind, is a *thought* of these unimaginable objects, i.e., a mental act. It is not clear to me what can be meant by an idea which is more than an image, over and above either an imageless experience of knowing or thinking, if there is such a thing, or an image used in a certain way for the purposes of thought. The former would certainly give us a mental act, and the latter would also, since it is a mental act to use an image in thought. Unless we are going to half-personify ideas and treat them as capable of indulging in "ideal self-development" of their own accord I do not see how we can avoid admitting mental acts over and above ideas, though it is an arguable view that there are no mental acts as *elements* in our experience and that what are referred to under that name are really Gestalt-properties of whole strands or complete systems of experiences. The positive arguments (pp. 411-414) which the author gives against the admission of mental acts, apart from the allegation that he cannot discover them in introspection, are arguments not against mental acts as such but against the direct theory of perception which is often combined with the admission of such acts, and they would lose their sting for anybody who admitted both images and sense-data dependent on the mind and mental acts.

Having dismissed the attempts of Associationists, Neutral Monists, Behaviourists, and Realists to replace ideas by images, physical reactions, or mental acts, and of Pragmatists to interpret thought too exclusively in terms of verification by future events and practical action, Prof. Blanshard turns with somewhat greater sympathy to "Critical Realism", according to which we can directly grasp the character of objects but not their existence. He contends, however, that the claim to direct knowledge of character is as incompatible with the facts of error and illusion as would be the claim to direct knowledge of existence, and that we cannot regard the content of our ideas as a set of timeless universals. More light, he thinks, is thrown on the question by Bradley's distinction between the two aspects of ideas as psychical fact and as conveying meaning, though the distinction is sometimes wrongly stated by Bradley.

This leads up to Prof. Blanshard's own theory of meaning and knowledge. He holds that "the relation between idea and object must be conceived teleologically, as the relation of that which is partially realized to the same thing more fully realized. When we say that an idea is *of* an object, we are saying that the idea is a purpose which the object alone would fulfil" (p. 473). One main motive for this theory is that it seems to satisfy both our inclination to regard knowledge as the attainment of identity with what is known, an idea which has led people to adopt views like the copy theory, a copy being the next best to an identity which seemed impossible, and our inclination prompted by obvious empirical facts to say they are quite different. Another is to account for the fact that the fulfilment of a subjective need gives us at the same time objective truth, a coincidence which without such an explanation would seem a miracle (p. 491). The theory depends on the acceptance of the category of potentiality, in terms of which the identity is to be understood, for it is obviously not actualized to any extent at the early stages and not completely at any stage of thinking about the object, and this relation of potentiality to actuality is identified with that of unrealized to realized purpose. The relation of an idea to its object is that of purpose to what would fulfil the purpose, and in so far as the purpose is fulfilled and the object known the idea actually passes into its object. The theory agrees with the common-sense supposition that ideas are like their objects but claims to analyze the likeness in the only possible intelligible way as the likeness of that which exists in potency to the same thing actualized. It agrees with neo-realism in holding that we know things themselves and not substitutes for them, it agrees with critical realism in holding that we are directly aware of their character while the object itself falls beyond (as an unrealized ideal), and it agrees with pragmatism in its emphasis on the purpose and character of thought and the instrumental function of ideas. It may thus claim to have taken the best out of all the current theories, though I think the argument becomes rather far-fetched when an attempt is made to find affinities also with behaviourism on the ground that the latter has been forced to have recourse to "motor-sets", i.e., something potential. In the course of developing the theory, as elsewhere in the book, Prof. Blanshard insists on the radical difference between teleology and mechanism (pp. 482 ff.), and he contends that the "emergent theory" is not adequate to meet the case, for while we are convinced that certain mechanically unpredictable things happen because we want them, the emergent theory merely says that they happen because the pattern has become so complex that they follow necessarily.

The epistemological theory expounded is similar to that of Royce, though developed differently. While I find it extraordinarily difficult to take seriously the notion of identity between the idea and its object, I am aware that the only alternative is to take the

cognitive relation as indefinable, which will excuse us from answering awkward questions. This may seem a cheap way out, but it is logically necessary that some things should be indefinable, and if anything is we should expect knowledge or truth to be so. We cannot show it to be logically impossible to know things about an independent reality directly, and if we thus know anything we surely know that what we must think (in the logical sense of "must") must be true of the real. When we turn away from other alternatives to the author's own view various objections present themselves. We cannot define knowledge as just identity, it obviously is not knowledge unless the identity is recognised, *i.e.* known, and therefore the danger of a vicious circle looms close at hand. Prof. Blanshard would no doubt reply that the definition was not in terms of mere identity but of purposed identity. We mean A when our idea is purposively directed towards identity with A, and we know A when this purpose is satisfied. However, it may still be objected that purpose already presupposes knowledge, and though it would be contended by many that this is not true of all purpose it is clearly true of the conscious purpose which could alone be used in defining knowledge. It would not be sufficient to constitute knowledge if the purpose towards identity just happened to be fulfilled without something additional, *i.e.*, our recognizing (knowing) that it was fulfilled. Secondly, as the author admits, the identity is never completely attained, so the only way of distinguishing knowledge from error would have to be by saying that the purpose towards identity was less completely attained in the latter case than in the former. But is this not to define knowledge in terms of resemblance? I do not see how greater or less attainment of a purpose towards identity which will never be completely fulfilled could be measured except in terms of resemblance. The purpose must be to approximate to identity, *i.e.*, to resemble, and does not this bring one back to the copy theory of knowledge which is rightly repudiated by Prof. Blanshard? He admits likeness between ideas and their object, but claims that his theory gives the only intelligible account of this likeness as being "the likeness of what exists in potency to the same thing actualized" (p. 495), and this might be all right if the idea ever were completely actualized as its object so that it was numerically the same, but since this does not happen we cannot define the likeness of the one to the other in terms of becoming the other since the one never becomes the other but only becomes more like the other. Nor is the illustration in terms of art altogether helpful; he claims that Shakespeare is a great dramatist just because he almost is what his characters are, or because his thought comes nearer to being a reliving of the very feeling itself (p. 551); but if so, why is not everybody a great poet when he expresses his own feelings at the time he is having them, and how is it that we wish to represent dramatically or enjoy the dramatic representation of experiences that we should avoid at all costs in real life? I give

these objections, but I am not confident that Prof. Blanchard could find no way of answering them, still less that his theory does not express (though I think inadequately) an important aspect of the process of thought, but the questions must be put all the same. If this theory of thought entails, as he thinks, the theory of internal relations, the coherence theory of truth, and the assimilation of causal to logical necessity, this will constitute a strong additional argument against it to the mind of many philosophers.

Prof. Blanchard now turns to the problem of universals. He effectively criticises the doctrine of abstract universals of which he thinks modern symbolic logic has not, any more than Aristotelian logic, shaken itself free. He argues that Johnson's attempt to save generic universals by making the circumstance on which they depend not a common quality but a special kind of difference breaks down on the ground that (1) the mode of difference itself presupposes the genus which it is intended to define; (2) if Johnson was right, the genus, *e.g.*, colour, would itself be that mode of difference, but it would be nonsense to say that colour is a way of differing from other colours; (3) "the abstract universal colour was rejected because no such common element could be discovered, but unfortunately the way in which colours differ is just as incapable of isolation as the way in which they agree" (p. 595). Prof. Blanchard then applies his theory of knowledge and reduces the distinction between generic universals and particulars to a distinction between vague and definite apprehension of the same thing. His theory sounds at points like a denial of the reality of universals altogether, but this impression is misleading. As we read on, it becomes plain that he is concerned in his criticism only with generic universals, not with specific universals such as a particular shade of colour or the number three. So far from wishing to deny these, he argues that particulars are reducible without residuum to universals of this type. But in regard to generic universals he thinks his doctrine the only one which can avoid both the difficulties of the abstract universal and the mere identification of the universal with a set of particulars. He unfortunately does not explain how his own theory would avoid one of the main criticisms which he brings against the doctrine of the abstract universal, *i.e.*, that it leads logically to the conclusion that the more universal a concept is the emptier it is. He could no doubt have met that criticism by distinguishing between a class concept and knowledge of the varying properties which belong to the different members of the class, but could not the defenders of abstract universals have met the criticism in the same kind of way? However, I do not want to defend the view that there is a distinguishable universal character such as, *e.g.*, colour in general. But I think the author's view ought to have been further elaborated with reference to particular processes of thought. It seems to me that the psychological account of thought which he gives might well have been written by somebody

who held a quite different view of universals. The argument that particulars are reducible to universals (of the specific, not the generic, type) is exceedingly well done.

Book III (the first part of volume II) gives a general account of the actual process of thinking. The first seven chapters should be regarded as a continuation of the psychological account of perception in Book I (the first part of vol. I). The author's main theme is the necessity of introducing something beyond association even at low levels of mental development and of exhibiting thought as the working of an immanent ideal of coherence. Even where work seems to be done by what is often called the subconscious, this is only made possible, he insists backed by many examples of works of genius, by the previous exercise of conscious intelligence. There follows a masterly defence (the best I have seen) of the coherence theory of truth. The argument against the correspondence theory and against the notion of merely given facts is of the usual type but freshly stated. The view which makes self-evidence the criterion is rejected on the grounds that it at any rate *increases* the certainty of axioms to argue that they are presupposed in a vast quantity of other presumptive knowledge and therefore they cannot be already completely self-evident apart from that argument, that the degree of certainty in our propositions does in fact vary in the way in which it would if coherence were the criterion, that alleged self-evident propositions have not, as one would expect if they were really self-evident, been reached by discontinuous leaps but by painful gropings and gradually, and that even the laws of logic have been questioned by some competent thinkers which, even though their doubts are mistaken, shows at least that the laws are not absolutely self-evident. The author accepts my own argument that the application of the coherence test to anything itself requires an immediate insight at some point or other to see that it conforms to the test, so that in a sense self-evidence is still involved (p. 258); but he is protesting against the notion of self-evidence as a property of propositions giving them certainty apart from the system. He would say that when something rightly appears self-evident it is because of the immanent working in our mind of the idea of the system. The technical objections to the proof of the laws of logic by the "This or Nothing Argument" he meets by contending that the argument is not that if we deny, *e.g.*, the law of contradiction all propositions would be false but that no propositions could be asserted at all (pp. 254-257). In general the argument against other tests is that they can do nothing of themselves unless backed by coherence. To the objection that it should follow from the coherence theory that all new theories must be rejected on the ground that they are less coherent with well-established beliefs than the old theories he replies that, if good, the new theory coheres better with established beliefs about the technique of acquiring beliefs than the old one which it opposes, *i.e.*, it contradicts old beliefs on

grounds which, if rejected in this case, would have to be rejected in others, and it is on these very grounds that the old beliefs themselves depend. *E.g.*, if the evidence of observation under scientifically accepted conditions was dismissed as illusory in order to secure coherence with the old beliefs we should in consistency have no right to rely on such evidence anywhere and this would destroy the ground also for the old beliefs themselves (pp. 284-286). Other objections are avoided by insisting that it is coherence together with comprehensiveness which is the test and not merely coherence apart from our experience.

The belief that coherence constitutes not only the criterion but the nature of truth follows from the author's general epistemological theory which must identify the subjective with the objective end, but he also argues that if the criterion and the definition of truth did not coincide we should have no means of telling that the former was a criterion of truth. This question raises serious difficulties, but I am not certain that it would be impossible to show that a more coherent theory could be produced if we admitted that truth itself consisted in correspondence than if we insisted on making coherence not only the criterion but the definition of truth. But however that may be, the author is right in insisting that the acceptance of coherence as the test implies that reality itself is a coherent system and therefore leads to important consequences in metaphysics. For if reality were incoherent, an incoherent and not a coherent set of propositions would be true. The author has also a very interesting chapter on the doctrine of degrees of truth, which is ably defended.

Book IV is devoted to the question of logical necessity. The author holds the unpopular view that this is not confined to certain elements in the *real* but is present in what is essentially the same form everywhere, and he makes an exceedingly able attack on the rival doctrines of empiricism, formalism, logical positivism and the external theory of relations. Traditional empiricism with its attempt to derive the supposed *a priori* from repeated experience and logical positivism which seeks to make all *a priori* propositions analytic or at least explains them in terms of linguistic conventions are crushingly refuted. I particularly welcome the attack on the exclusively extensional interpretation of propositions characteristic of so much in modern logic. The attack on "formalism", the view that necessity belongs only to certain forms as such, is not so clearly effective, largely because the latter is intrinsically a less untenable position, but the author raises difficulties which at the very least compel a restatement of the doctrine. After giving examples of *a priori* propositions where the content of the terms is not irrelevant to their truth he contends that it is absurd to make even syllogisms purely formal. "When we grasp that Socrates, being human, must die, it is ridiculous to say that the subject of which we see this is not Socrates at all, but Sness, or the abstract property of being a term" (p. 370). The formalist would no doubt

reply that we do grasp that Socrates, not Sness, is mortal, but only through grasping the form of the proposition; but this account involves the doctrine of abstract universals which Prof. Blanshard claims to have refuted, and it does seem to me that such a doctrine of logic will be in a very insecure position unless and until it can be shown that it is compatible with some more tenable theory of universals. On the other hand I do not see how Prof. Blanshard's own doctrine of generic universals as merely an inadequate thinking of particulars would fit in with the precision, necessity and certainty which belong and belong specially to some of the most universal propositions we can conceive. The attention of symbolic logicians should be called to the criticism of Lewis' definition of "strict implication" with which the chapter closes.

Leaving these ingenious, if perverse, attempts of logicians to elaborate a system of logic from which all necessity is excluded, Professor Blanshard after an all too brief defence of the view that all necessity is of the same kind and that necessity has degrees, turns to the question of relations. The large space he devotes to this question is justified by the fact that what is usually called the theory of external relations would, if adopted, be in fatal conflict with his whole theory of knowledge. A relation is said to be "internal to a term when in its absence the term would be different" (p. 451). The view that all relations are internal has been attacked on the following grounds. (1) Science depends on our being able to distinguish what is relevant from what is irrelevant, and therefore presupposes that everything is not relevant to everything else. (2) If terms changed their meaning according to their context all reasoning would be invalidated, *e.g.*, all syllogisms would have four terms. (3) It is obvious that a characteristic like, *e.g.*, threeness is the same whatever things it qualifies. Prof. Blanshard replies to the first objection by recognizing degrees of relevance throughout and even absolute irrelevance *within a particular limited system*. In reply to the second objection he launches a counter-attack and argues that on the contrary his own view is essential if we are to make any sense of reasoning. To suppose that a term can remain quite the same in different contexts is, he contends, to accept the untenable doctrine of the abstract universal. If, for instance, in the well-known syllogism about mortality we abstract from the nature of an organism everything not common to all organisms, we shall have nothing left capable of forming the basis of an implication. He admits, indeed, that abstract universals are necessary to the work of thought, but only as an inadequate means. "The method of abstract analysis is applicable to the wholes in the degree to which their parts are ununified (in the sense of being related externally), and *not* applicable in the degree to which they are unified, since none of the wholes of actual experience are mere and pure aggregates, it is *perfectly* applicable nowhere, but since none of them are ideal unities either, it is *in some measure*

applicable everywhere ; finally, since the ideal of thought is a system completely unified it will hold *nowhere in the end* " (p. 469). This brings out the central importance, which I had not myself realized before reading the book, of the problem of universals. While sympathetic with the author's contention I should have liked a more definite answer to the question in what exactly the identity of the middle term required by a syllogism or other arguments consists. To the third argument he replies that numerical characteristics are independent only because the units have been artificially defined in a way which ignores specific differences.

He then examines the argument for the internal relatedness of things based on causality. Taking for granted that everything is causally determined (he curtly dismisses the doubts on this question inspired by science and does not even discuss those raised by the problem of freedom) he proceeds to consider the question whether causality involves an element of necessity in the logical sense. To this he returns an affirmative answer with which I am naturally in sympathy as it is backed by the same type of arguments as I myself have used in another place. The argument from the possibility of induction is put very effectively when he contends that the uniformity of nature is the *principle* of inductive arguments and the *principle* of a valid argument cannot be without necessity (pp. 507-508). The argument from causality is of extreme importance to him because it leads to the conclusion that the ideal of intelligible system is not only a fancy but actually realized since everything we know seems to be causally connected with everything else and causation has, he claims, now been shown to involve logical necessity. He indeed gives here a misleading impression as if the whole weight of the argument for his view of the nature of the real rested on causation and the arguments based on the nature of relations, whereas in fact his earlier argument for the coherence theory of truth is of itself a powerful argument for the view that reality, *i.e.*, that to which true judgments refer or at which they aim with a success in proportion to their truth, is a coherent system in the sense in which coherence serves as a criterion and, according to him, a definition of truth.

He is, however, careful to say that there can be no question of "proof" here where the very nature of proof is at stake. "In the last resort, all we can say to the doubter is, Here is the standard and ideal that for our own part we seem to be using in the practice of thought ; is it not yours also ? When you have seemed to understand, has it not always been through system ? As your understanding grows more complete, is it not always through a system of greater unity, in the sense explained ? . . . We read *The Origin of Species*, and man's place in nature is transformed for us. We have gained a flood of light, of indubitable intellectual light. How much of it would be left as light if, following the lead of formalism, we denied all degrees to necessity, and confined it to a nexus of the

barest abstractions? . . . Or perhaps in a poem, or a piece of music, or one of the varieties of religious experience, an insight is gained that one hesitates to call understanding, because understanding has come to be thought so abstractly intellectual an affair. On our view it is understanding, special in kind, to be sure, and limited in degree, but still with rights of its own; and to reject it as understanding on the ground of its concreteness is perverse. If you accept the ideal of concrete necessity these varying insights are legitimized, ordered, and appraised. Accept any other whatever, and, against what seems to us your better judgment, some must be excluded" (pp. 518-519). I have quoted this passage at length because it sums up better than I can the main message of the book. It remains to close the book with a regret which, considering the length of the work, is itself a very decided compliment to the author, and to thank him heartily for having given us what I think is one of the best and most important books written in the last few years in the field of philosophy.

A. C. EWING.

V.—NEW BOOKS.

A Modern Elementary Logic. By L. SUSAN STEBBING, D.Lit. London : Methuen & Co. Ltd., 1943. Pp. vi + 214. 8s. 6d.

IN this book, published shortly before her death, Miss Stebbing has rendered a useful service to first year students who are reading elementary logic for a University examination. The bulk of her space is taken up with questions concerning deductive logic, but there is a short chapter, admirably planned, dealing with the methodology of science. For his main work on this topic the student is referred to other books. Since she had examinations in view, she was compelled to introduce into her treatment of formal logic some topics which she regarded as technical trivialities, though the space given to them is cut to a minimum. A book with such a purpose must deal with a certain conventional range of topics, must introduce certain technical terms, must contain a number of exercises, and under present conditions should contain hints for their solution. All this Miss Stebbing faithfully gives us. Her treatment of topics is thoroughly up to date, and any student who has mastered this book will be in a position to start right away on any of the modern larger treatises on the subject. She writes with her usual vigour and good sense, and with her usual interest in modern affairs; and the result is a book which can be thoroughly recommended to anyone beginning the subject. She is never dull. Her point of view is, of course, that of her larger work, but she keeps her range of topics to what can be expected of a student beginning the subject.

A few minor points may be noted. On p. 37 we are given as the significant obverse of "No snobs are welcome guests", "All snobs are unwelcome guests". This seems to me unsatisfactory. It would be wiser either to keep to the formal "not—(welcome guests)", or to note that a significant proposition which corresponds to this would be, "All snobs are either not guests or unwelcome guests". A consequence of her treatment is seen on p. 40, where an inverse of "All honest politicians are mortals" is given as "Some dishonest politicians are immortals". On p. 83 "next to" is given as an "intransitive" relation; it should be "non-transitive", as a consideration of the legs of a tripod shows. Two lines further down, " yRx " should be " yRz ". The statement about H.C.F. on p. 87 needs to be reworded. On p. 97 the validity of inferences of the form " $aRb . bRc, \therefore aRc$ " is spoken of as *depending on* the property of transitivity, while on p. 83 transitivity is defined in terms of the validity of the inference. The point intended is better put at the foot of p. 97, where the traditional logicians are spoken of as "failing to single out the property of transitivity as essential to such inference". I am a little perplexed by the statement on pp. 101-2 that the denotation of a term is not the *class* but the *collective membership* of the class; this latter phrase may become an incantation if the student doesn't quite get its meaning. The abstract word "membership" is the sinner. The statement on p. 113 mid. about *species* seems to lead to the consequence at the foot of the page that it is an accident of the *species* circle to be inscribed in a triangle. The point expressed by the statement on p. 114 that "Accidental predicates are predicated *not* of an individual but of an individual *as a member of a species*" would be more clearly expressed by saying that predicates of an individual are described as accidental only when the

individual is considered as a member of a species. On p. 119 a chiliagon is described as a thousand-sided regular polygon: "regular" should be "plane". On p. 168, line 5, " ϕ " should be " ψ ".

These are small points, and of little importance. Many of them would no doubt have been altered in the final revision had her health permitted. They do nothing to detract from the value of the book.

Miss Stebbing will be greatly missed. As a writer, she was vigorous and incisive, but this was in no way inconsistent with her fundamental humility as a thinker. She was intolerant of what she took to be pretentious, but with learners she was both patient and kindly. Her range of interests was wide, and her fair-mindedness, and her readiness to appreciate points of view in philosophy different from her own, made her both an excellent teacher and an admirable external examiner. She suffered much from ill-health, and most of her writing was done amid great pressure of other work; but she gave her time generously to those around her who were in need of help, and some of her dearest friends during their illness knew how ready she was to spend long hours at their bedside. She was a woman of deep feeling, and the unhappiness of others weighed on her heavily. She was convinced that to get rid of evils people must learn to think clearly; and it was this conviction that inspired all her work.

L. J. RUSSELL.

A Study on Punishment (English Studies in Criminal Science, Pamphlet Series). 1. *Introductory Essay*. By L. RADZINOWICZ and J. W. C. TURNER. 2. *Punishment as Viewed by the Philosopher*. By A. C. EWING. Toronto: Canadian Bar Association; Cambridge: The Squire Law Library, 1943. Pp. 32. 2s.

THE editors of this series, in introducing the philosopher's view of punishment to the lawyers, quote Blackstone's statement that "Sciences are of a sociable disposition, and flourish best in the neighbourhood of each other". Certainly philosophy flourishes best when associated with other disciplines, and moral philosophy not least when associated with jurisprudence. Moral philosophers have in the past appreciably influenced the jurists, and students of ethics have much to learn from law.

The introductory essay to this pamphlet is a brilliant little survey of changes in the conception and treatment of punishment in English criminal law in the nineteenth and twentieth centuries, and contains much to interest the philosopher. If I might pick out one point for special mention, it is that the jurists, like some philosophers, are moving away from punishment proper. "There is no good reason for insisting that probation [which is now used for a large proportion of offenders] is punishment", says a recent writer on criminology quoted here: and Ferri in his draft penal code of 1921, we are told, "abandoned the use of the traditional word 'punishment' and substituted for it the expression 'Measures of Social Defence'".

Amid these changes the editors of this series of Studies, which are published under the auspices of the Department of Criminal Science in the University of Cambridge, felt it would be useful to call in the philosopher to assist the jurist, and invited Dr. Ewing to give a "survey of the main currents of modern philosophical thought in England on the subject of punishment". Dr. Ewing outlines the utilitarian approach, with its

aspects of deterrence and reform, and the retributive view. He summarises the recent statements of Sir David Ross, Mr. E. F. Carritt, Mr. J. D. Mabbott and Mr. W. G. MacLagan, who all say that in their views punishment is in essence retributive. From them Dr. Ewing draws the conclusion that the utilitarian view must be modified so as to take some account of justice in that (a) punishment of the innocent is wrong in itself whatever the consequences, and (b) a more severe punishment is required for a worse crime. Although he is at greater pains than the "retributionists" to stress utility, in effect his view would, I think, find general agreement among contemporary philosophers. Shortly, this view is that punishment of the innocent is wrong in itself and that punishment of the guilty, in itself permissive but not obligatory, is to be justified by its consequences.

In looking at the different kinds of consequences which help to justify punishment, Dr. Ewing stresses the reform of the criminal rather more than the prevention of crime. He agrees that much reform is not punishment, but the essential nature of punishment, he thinks, is to be found in its reformatory and educative effects. "The primary object of punishment is to lead both the offender and others to realize the badness of the act punished." "Punishment is", according to Dr. Ewing, "essentially an expression of moral condemnation" or "a kind of language intended to express moral disapproval". In a more sensitive society, he holds, a verbal expression would be sufficient to achieve the purpose of punishment. (What would then happen to the dictum, which Dr. Ewing accepts, that a more severe punishment is required for a worse crime? Would we say "Naughty, naughty" to the thief but swear violently at the murderer?) Dr. Ewing reaches his definition from a consideration of Mr. Carritt's view, but seems to forget that, as he has told us earlier, Mr. Carritt holds that the only fully appropriate punishment is remorse. If Mr. Carritt is right Dr. Ewing's definition is faulty, for *remorse* cannot be called a kind of language. Perhaps Dr. Ewing should say that there are two senses of the word "punishment", a primary sense in which it means consciousness of, and repentance for, having done wrong, and a derivative (and more common) sense in which it means causing, or attempting to cause, punishment in the first sense by the expression of condemnation.

It may possibly be right to define "punishment" in one of its most common uses as meaning the expression of moral condemnation for wrongdoing. It is no doubt also true that in an enlightened penal system reform and education are among the aims intended. But reform is certainly not to be achieved by the mere *expression* (whether by words or whips) of condemnation for crime; explanation of why the crime is a crime, and remedial measures resulting from a study of the causes which have made the particular criminal a criminal, are more potent aids to reform. I do not think that Dr. Ewing's definition is to be derived from the reformatory aspect of punishment.

Dr. Ewing also says some useful things about punishment in practice and the dangers of such forms of punishment as imprisonment. He thinks that more attention should be paid to reparation, and suggests that the notion of retribution persists through a confusion with reparation.

D. DATCHES RAPHAEL.

In Commemoration of William James, 1842-1942. New York : Columbia University Press ; London : H. Milford, 1942. Pp. xii, + 234. 18s. 6d.

WILLIAM JAMES was born on 11th January, 1842, and American philosophers, despite the war, have celebrated the centenary of his birth with a solicitude almost adequate to their debt to his genius. The present volume consists of three sets of addresses at philosophical congresses—a set of six in New York City, of four in Poughkeepsie, and of three in Madison, Wisconsin—and of three more lonely stragglers. At New York, Malinowski was to have addressed the conference on James's attempt to find a substitute for the heroism of war, but he died first, and the missing address is replaced by an essay on the same subject by Mr. Bixler. It originally appeared in the *Harvard Theological Review*. H. M. Kallen contributes a foreword to the volume.

Compared with most of its kind this book, I think, is quite exceptionally interesting ; and it is very interesting absolutely. James's many-sidedness, I suppose, had something to do with this, and also the lively interest that is still taken in his fresh, clever, winning pages. That, however, is not a complete explanation of the marked success of this commemorative venture. As well as the authors, the planners of the enterprise must take their share of the praise. For one important thing, they have not over-planned the book. The contributors do not collaborate as a team, but, as it were, fill a gallery in which there is a pleasing disorder. Each speaks his own mind, sometimes saying what some others say (and even citing the same evidence), sometimes unsaying what some of the others say, though not by way of intentional opposition. Mr. Metzger, for instance, regards James's philosophy as a valiant American Yea affirming deliverance from the suffering of European culture, and James himself as a Nietzsche with a big but intelligible difference. Contrariwise, Mr. D. C. Williams finds that James was "immured in the contemporary" and "parochial in time" even more than most philosophers, and that he was preoccupied with a philosophical bubble of absolute idealism which drifted across the horizon of a "world of complacent bourgeois intellectualist liberalism". That is an extreme instance, but if one compares Mr. Dickinson Miller's account of his debt to James for showing him quite clearly why there is such a thing as pure phenomenalism and what it is, with Mr. E. B. Holt's essay, cheek by jowl with Miller's, attempting to show how James "searched for and found" the way to establish a motor psychology with a reconstituted physiological basis, and then compares both these essays with Mr. Kantor's essay hailing James as the forerunner of an "interbehavioural field psychology" which supplants and subordinates both phenomenalism and physiology, the lesson to be learned is scarcely less extreme.

Mr. Perry has written, not so very long ago, so full and so admirable a biography of James that few of the contributors have borrowed or added much biographical material although the biographical exordium to Mr. Lyman's contribution crowds the rest of his observations out, and although Mr. Schneider skilfully uses biographical material in his valuable essay upon "James as a Moralist". In a more oblique way, however, the volume gains in interest from the difference in attitude between those who knew James well or not so well, on the one hand, and on the other, the contributors, mostly younger, who didn't know him at all. Of the one part there are his son, friends and correspondents like Miller and Dewey, and his pupil and biographer, Perry. Of the other part there are strangers like Metzger, or (as I should judge) younger men such as Lowe or Williams.

James as psychologist, pragmatist, empiricist, moralist, James as "dramatic empiricist", as Mr. Charles Morris calls him in his essay, James as philosopher-psychologist-moralist—these themes, quite naturally, are the most fully exploited. Mr. Lowe, more unusually, very courageously and very skilfully treats James as a metaphysician. James's superabundant vitality has communicated itself to a high proportion of these addresses largely because most of the essayists, having a good deal of vitality themselves, are eminently stimulable.

My own reactions to these essays have little, if any, general interest, and, I daresay, will not be shared by very many. Such as they are, however, I shall set them down. The two essays which caught and held my attention best were the two which, on the whole, were most critical of James, namely Bixler's and Williams's; but Lowe's ran them hard. I suppose I found a quality of unexpectedness in these essays, being better prepared in advance for many of the others. If so, the thing is rather crudely subjective, and, very likely, a more delicate satisfaction might have been mine, had I preferred to notice how skilfully so many of the others presented a case that was not and could not be unexpected.

JOHN LAIRD.

The Theory of Motion in Plato's Later Dialogues. By J. B. SKEMP, M.A.
Cambridge University Press, 1942. Pp. xv + 123. 8s. 6d.

THIS is an interesting and valuable monograph on Plato's theory of motion. It deals particularly with the *Timæus* which Mr. Skemp regards as the most authoritative exposition of the doctrine. He will not allow the same degree of authority to the argument in Book X of the *Laws* which he regards rather as a popular summary of the views set out more seriously in the earlier dialogue. But he treats of the other dialogues, and in addition gives us an erudite discussion of the antecedents of Plato's doctrine in the work of earlier thinkers, particularly Alcmaeon and Empedocles. The exposition, as a whole, is marked by patient learning and sound critical judgement. It is not, I think, as clear as it might be in the development of the argument. This is partly because Mr. Skemp's conscientiousness makes him a little too ready to break the thread of his own argument in order to deal in passing with some other view which he does not accept. But there also seems to me to be a certain lack of decisiveness on some points, and on others I do not think that Mr. Skemp fully appreciates the difficulties that face his interpretation.

However, on some of the main points he is clear enough, and in most of these his views seem to me thoroughly sound. He maintains that the theory of motion is as integral a part of Plato's final philosophy as the theory of Forms, that it is to be looked for, in the main, in the *Timæus* which represents, though in mythical form, Plato's own view, and that soul, the cause of motion, is an element in ultimate reality which exists in its own right and cannot be reduced to or derived from the Forms. With all this I am in the fullest agreement.

About the further details of the argument I am much more doubtful. If I understand Mr. Skemp rightly—and I am not quite sure that I have done so—he maintains that, besides the Demiurge, who is soul completely informed by *νοῦς*, there are, as independent existences, not only space, the matter of the material universe, but also *ψυχὴ δόμιος*. This latter appears to be regarded as, in some sense, the matter out of which the

Demiurge creates or constructs the soul of the material universe. It is what Plato means by the *πλανομένη αἴρια* and by *ἀράκη*, and is responsible for random movement before the world is reduced to order by the Demiurge and for rectilinear movement, or perhaps all except circular movements, afterwards. Up to a point, in fact, Mr. Skemp appears inclined to follow Plutarch. I cannot find this convincing, and Mr. Skemp does not seem to me to dispose of the difficulties though he mentions some of them. The issue cannot be discussed properly without presenting a complete alternative exegesis. But I would venture one suggestion. One may agree with Mr. Skemp that the *Timæus* is the central exposition of Plato's view on these points. But the *Timæus* is expounded in the form of a myth, which needs a good deal of interpretation. I should have thought, therefore, that the other dialogues, particularly the *Laws*, deserve to be taken more seriously than they are by Mr. Skemp, not as correcting or modifying the *Timæus*, but as the key to what that dialogue means.

G. C. FIELD.

Received also :—

- J. S. Huxley, *Evolutionary Ethics* (The Romanes Lecture, 1943), Oxford University Press, 1943, pp. 84, 2s.
- L. Hodgson, *Towards a Christian Philosophy*, London, Nisbet & Co., 1942, pp. 195, 10s. 6d.
- A. D. Lindsay, *Religion, Science and Society in the Modern World*, Oxford University Press, 1943, pp. 64, 3s. 6d.
- I. Düring, *Aristotle's De Partibus Animalium : Critical and Literary Commentaries*, Göteborg, Wettergren & Kerbers, 1943, pp. 223, Kr. 10.
- H. Rackham, *Aristotle's Ethics for English Readers*, Oxford, Basil Blackwell, 1943, pp. 176, 4s. 6d.
- P. A. Kristeller, *The Philosophy of Marsilio Ficino*, New York, Columbia Univ. Press ; London, H. Milford, 1943, pp. xiv + 441, 30s.
- K. R. Wallace, *Francis Bacon on Communication and Rhetoric*, The University of North Carolina Press ; London, H. Milford, 1943, pp. ix + 277, 30s.
- Ben-Ami Scharfstein, *Roots of Bergson's Philosophy*, New York, Columbia Univ. Press ; London, H. Milford, 1943, pp. ix + 156, 11s. 6d.
- G. F. Stout, *The Groundwork of Psychology* (Revised by R. H. Thouless. Third Edition, with additions by S. H. Mellone), London, University Tutorial Press, 1943, pp. viii + 244, 5s. 6d.
- A. H. Bowley, *The Natural Development of the Child* (Second Edition), Edinburgh, E. & S. Livingstone, 1943, pp. xvi + 184, 8s. 6d.
- H. S. Jones, *Copernicus* (The Selby Lecture, 1943), Cardiff, University of Wales Press, 1943, pp. 30, 1s. 6d.
- F. Shoemaker, *Aesthetic Experience and the Humanities*, New York, Columbia Univ. Press ; London, H. Milford, 1943, pp. xviii + 339, 23s. 6d.
- British Association for the Advancement of Science : *Report of the Committee on Scientific Research on Human Institutions* (Reprinted from "The Advancement of Science," No. 8), Burlington House, London, W.1, 6d.

MIND ASSOCIATION.

The following is the full list of the officers and members of the Association :—

OFFICERS.

President.—PROF. H. H. PRICE.

Vice-Presidents.—PROFS. C. D. BROAD, B. EDGELL, G. C. FIELD, A. D. LINDSAY, L. J. RUSSELL, J. W. SCOTT, N. KEMP SMITH, and G. F. STOUT.

Editor.—PROF. G. E. MOORE.

Treasurer.—MR. H. STURT.

Assistant-Treasurer.—PROF. B. BLANSHARD.

Secretary.—MRS. M. KNEALE.

Guarantors.—PROF. A. D. LINDSAY and SIR W. D. ROSS.

MEMBERS.

AARON (Prof. R. I.), University College, Aberystwyth.

ACTON (H. B.), Bedford College, London, N.W. 1.

ADAMS (Rev. A. W.), 260 Fulwood Road, Sheffield, 10.

AINSLIE (D.), The Athenæum, Pall Mall, S.W. 1.

ALBUQUERQUE (M. d'A.), 30 Rua do Conde, Ponta Delgada, Azores.

AMBEROSE LAZEROWITZ (Mrs. A.), 69 High Street, Northampton, Mass., U.S.A.

ANDERSON (Prof. J.), University, Sidney, Australia.

ANDERSON (Prof. W.), University College, Auckland, New Zealand.

APPLEBY (M.), Royal University of Malta, Valetta, Malta.

ARMSTRONG (A. McC.), 4 Church Hill, Edinburgh, 10.

ATLEE (Prof. C. M.), Dept. of Education, University College, Nottingham.

AYER (A. J.), Christ Church, Oxford.

BARKER (H.), 10 Cairnmuir Road, Edinburgh, 12.

BARTLETT (Prof. F. C.), St. John's College, Cambridge.

BAUER (G. A.), 27 Clanricarde Gardens, London, W. 2.

BECKER (Prof. F. C.), Lehigh University, Bethlehem, Pa., U.S.A.

BEDFORD (E.), 15 Vaughan Gardens, Ilford, Essex.

BEECH (F. P.), National Provincial Bank, Wrexham.

BEGG (J. C.), 12 Fifield St., Roslyn, Dunedin, N.Z.

BENNETT (E. S.), British Legation, Peiping, China.

BENNETT (J. G.), Coombe Springs, Coombe Lane, Kingston, Surrey.

BERLIN (I.), New College, Oxford.

BLACK (Prof. M.), 702 Nevada St., Urbana, Illinois, U.S.A.

BLANSHARD (Prof. B.), Swarthmore College, Swarthmore, Pa., U.S.A. *Life Member.*

BLEVIN (W. P.), 19 Kylemore Drive East, Pensby Road, Heswall, Cheshire.

BOODIN (Prof. J. E.), University Club, 614 South Hope Street, Los Angeles, Calif., U.S.A.

BOYNTON (Prof. R. W.), The University of Buffalo, N.Y., U.S.A.

BOYS SMITH (Rev. J. S.), St. John's College, Cambridge.

BRAITHWAITE (R. B.), King's College, Cambridge.

BRETT (Prof. G. S.), The University, Toronto, Canada.

BRIGHTMAN (Prof. E. S.), Box 35, Newton Center, Mass., U.S.A.

BRITTON (K.), University College, Swansea.

BROAD (Prof. C. D.), Trinity College, Cambridge.

BROSNAN (Rev. J. B.), Sylfield, Lord St., Westthoughton, Lanes.

BROWN (G.), 48 Lilybank Gardens, Glasgow.

BROWN (Dr. W.), Christ Church, Oxford.

CAMPBELL (Prof. C. A.), The University, Glasgow.
 CAMPION (G. G.), Inglegarth, Bramhall, Cheshire.
 CARNAP (Prof. R.), Box 307, Route 2, El Paso, Texas, U.S.A.
 CHAPMAN (H. W.), Pedlar's Oak, Ivy House Lane, Berkhamsted.
 CHARLTON (143976 P./O. E. H. D.), c/o 359, W.4, R.A.F., India Command.
 COIT (Dr. S.), 30 Hyde Park Gate, London, S.W.
 COOMBE-TENNANT (A. H. S.), 18 Cottesmore Gardens, Victoria Road, Kensington, W. 8.
 COPP (Miss M. T.), Wilson College, Chambersburg, Pa., U.S.A.
 CORY (Dr. D. M.), 527 West 113th Street, New York, U.S.A.
 COUSIN (D. R.), 7 Lorraine Gardens, Kelvinside, Glasgow, W. 2.
 COX (H. H.), Lincoln College, Oxford.
 CRAWLEY (Mrs. C. W.), Ladies' Park Club, 32 Knightsbridge, S.W.
 CROSS (Rev. F. L.), Pusey House, Oxford.
 CROSS (R. C.), Jesus College, Oxford.

D'ARCY (Rev. M. C.), Campion Hall, Oxford.
 DENNES (Prof. W. R.), University of California, Berkeley, U.S.A.
 DESSOULAVY (Rev. Dr. C.), 171 Fentiman Road, S.W. 8.
 DIXON (J. L.), Top Bents, Long Grove, Seer Green, nr. Beaconsfield.
 DORWARD (Prof. A. J.), The University, Liverpool.
 DUCASSE (Prof. C. J.), Brown University, Providence, R.I., U.S.A.
 DUMBELL (A. T.), Runnymede, Riverbank Road, Heswall, Cheshire.
 DUNCAN (A. R. C.), 11 Moston Terrace, Edinburgh, 9.
 DUNCAN-JONES (A. E.), The University, Birmingham.
 DUNNICLIFFE (Rev. E. F. H.), All Saints' Vicarage, Nottingham.

EDGEELL (Miss B.), Berryhead, Cleeve Hill, Cheltenham.
 EDWARDS (Rev. E. W.), Queensberry Court, 9 Queensberry Place, S. Kensington, S.W. 7.
 EMMET (Miss D. M.), The University, Manchester, 13.
 EWING (Dr. A. C.), 69 Hurst Park Avenue, Cambridge.

FARIS (J. A.), Rosebank, Marlborough Park, Belfast.
 FIELD (Prof. G. C.), The University, Bristol.
 FINDLAY (Prof. J.), University of Otago, Dunedin, New Zealand.
 FOSTER (M. B.), Christ Church, Oxford.
 FRANKS (Prof. O. S.), The University, Glasgow.
 FURLONG (E. J.), 6 First Avenue, Bushey, Herts.

GARNETT (Prof. A. C.), 191 Bascom Hall, University of Wisconsin, Madison, U.S.A.
 GILMOUR (J. S. L.), Descanso House, Royal Gardens, Kew, Surrey.
 GREENE (Prof. T. M.), Princeton University, N.J., U.S.A.
 GREGORY (J. C.), The Mount Hotel, Clarendon Road, Leeds, 2.

HAAS (V.), 111 Leicester Road, Blaby, Leicester.
 HALLETT (Prof. H. F.), King's College, London.
 HAMPTON (Prof. H. V.), S. T. College, Cruikshank Road, Bombay, India.
 HARDIE (C. D.), Gledstone Park, Bishopton, Renfrewshire.
 HARDIE (W. F. R.), 47 Palace Court, London, W.2.
 HART (H. L. A.), 21A Well Walk, Hampstead, N.W.3.
 HARVEY (Prof. J. W.), The University, Leeds.
 HAWKINS (Rev. D. J. B.), Karala, 66 Milbourne Lane, Esher, Surrey.
 HEADLY (L. C.), House on the Hill, Woodhouse Eaves, Loughborough.
 HENDERSON (G. P.), Manse of Tyron, Dumfries, Scotland.
 HOCKING (Prof. W. E.), 16 Quincy Street, Cambridge, Mass., U.S.A.
 HOERNLÉ (Prof. R. F. A.), The University, Johannesburg, South Africa.

HOOK (Prof. S.), Department of Philosophy, New York University, Washington Square College, Washington Square, New York, U.S.A.

HOPKINS (Prof. L. J.), 1385 Hillcrest Ave., Pasadena, Calif., U.S.A.

HOSPERS (J. J.), 528 International House, 500 Riverside Drive, New York City, U.S.A.

JACKSON (R.), The University, Edinburgh.

JESSOP (Prof. T. E.), University College, Hull.

JOHNSON (A. H.), University of W. Ontario, London, Canada.

JONES (Prof. D. James), St. Oswald's, Victoria Drive, Bangor, Wales.

KELLY (Rev. A. D.), Kelham Theological College, Newark-on-Trent.

KEYNES (Dr. J. N.), 6 Harvey Road, Cambridge.

KNEALE (Mrs. M.), Lady Margaret Hall, Oxford.

KNEALE (W. C.), Exeter College, Oxford.

KNOX (Prof. T. M.), The University, St. Andrews.

LANGFORD (Prof. C. H.), Philosophy Department, University of Michigan, Ann Arbor, Mich., U.S.A.

LAING (B. M.), The University, Sheffield.

LAIRD (Prof. J.), The University, Aberdeen.

LAMONT (W. D.), Dept. of Moral Philosophy, University, Glasgow.

LEAN (M. E.), 533 Livingston Hall, Columbia University, New York, U.S.A.

LEWIS (H. D.), University College of North Wales, Bangor, Wales.

LEWY (Dr. C.), Trinity College, Cambridge.

LIBRARIAN (The), The Aristotelian Society, 55 Russell Square, London, W.C. 1.

LIBRARIAN (The), Bedford College, London, N.W. 1.

LIBRARIAN (The), Girton College, Cambridge.

LIBRARIAN (The), Heythrop College, Chipping Norton, Oxon.

LIBRARIAN (The), The University, Jerusalem, Palestine.

LIBRARIAN (The), Lincoln College, Oxford.

LIBRARIAN (The), St. Xavier's College, Bombay, India.

LILLIE (Dr. R. A.), 267 Braid Road, Edinburgh.

LINDSAY (A. D.), The Master's Lodgings, Balliol College, Oxford.

LUCE (Rev. Canon A. A.), Ryslaw, Bushy Park Road, Co. Dublin.

MABBOTT (J. D.), St. John's College, Oxford. *Life Member.*

MACBEATH (Prof. A.), Queen's University, Belfast.

MACFARLANE (Sgt. C. A.), Schoolmaster's House, Yarpole, near Leominster, Herefordshire.

MCINTOSH (G. F.), 40 Hardy Street, Hurlstone Park, N.S.W., Australia.

MACIVER (A. M.), The University, Leeds.

MACKAY (Prof. D. S.), University of California, Berkeley, Cal., U.S.A.

MCKEON (Prof. R. P.), The University of Chicago, Chicago, U.S.A.

McKIE (J. I.), Brasenose College, Oxford.

MACLAGAN (W. G.), Oriel College, Oxford.

MACLENNAN (R. D.), Dept. of Philosophy, McGill University, Montreal, Canada.

MACMURRAY (Prof. J.), University College, Gower Street, London, W.C.

MACNABB (D. G. C.), Pembroke College, Oxford.

MALCOLM (Dr. N.), Dept. of Philosophy, Princeton University, N.J., U.S.A.

MALLET (E. H.), 14 St. James's Square, Bath.

MARSH (Rev. J.), Principal's Lodgings, Mansfield College, Oxford.

MELLONE (Dr. S. H.), 10 Hartington Gardens, Edinburgh, 10.

MILLER (Dr. E.), 7 Devonshire Place, London, W.1.

MOORE (Prof. G. E.), 86 Chesterton Road, Cambridge.

MORROW (Prof. G. R.), 310 Bennett Hall, University of Pennsylvania, Philadelphia, U.S.A.

MOTT (C. F.), Hope Lodge, Poplar Road, Oxton, Birkenhead.

MURE (G. R. G.), Merton College, Oxford.

MURRAY (A. R. M.), Woodend, Jedburgh, Scotland.

MURRAY (Principal J.), University College, Exeter.

NAGEL (Prof. E.), Department of Philosophy, Columbia University, N.Y. City, U.S.A.

NASON (Prof. J. W.), Swarthmore College, Swarthmore, Pa., U.S.A.

NELSON (Prof. E. J.), University of Washington, Seattle, U.S.A.

NORTHROP (Prof. F. C. S.), 1891 Yale Station, New Haven, Conn., U.S.A.

OAKELEY (Miss H. D.), 22 Tufton Court, Westminster, S.W. 1.

O'RAHILLY (Dr. A.), University College, Cork, Eire.

OSBORN (Sir F.), Mountcoombe Hotel, Oak Hill Grove, Surbiton, Surrey.

PARKER (Prof. D. H.), University of Michigan, Ann Arbor, Mich., U.S.A.

PATON (Prof. H. J.), Nether Pitcaithly, Bridge of Earn, Perthshire.

PEARCE (F. G.), 12 Holroyd Road, London, S.W.15.

PECK (A. D.), Trinity College, Oxford.

PHILLIPS (I. W.), Department of Moral Philosophy, The University, Glasgow.

PICKARD-CAMBRIDGE (W. A.), Worcester College, Oxford.

PORTEOUS (Prof. A. J. D.), 3 Kingsmead Road North, Birkenhead, Cheshire.

PRICE (Prof. H. H.), New College, Oxford.

RICHARD (Prof. H. A.), 6 Linton Road, Oxford.

RANADE (Prof. R. D.), The University, Allahabad, India.

RAPHAEL (D. D.), Oriel College, Oxford.

REID (Prof. L. A.), King's Coll., Newcastle-on-Tyne.

RHEES (R.), 96 Bryn Road, Swansea.

RICE (V. L.), c/o Mr. J. Hillard, 60 Argyll Road, East Kew, Victoria, Australia.

RITCHIE (Prof. A. D.), The University, Manchester, 13.

ROBINSON (Prof. R.), Cornell University, Ithaca, N.Y., U.S.A.

ROSS (Prof. G. R. T.), 62 Marine Terrace, Aberystwyth, Wales.

ROSS (Sir W. D.), Provost's Lodgings, Oriel College, Oxford.

ROTH (Prof. Dr. H. L.), The University, Jerusalem, Palestine.

RUSSELL (Rt. Hon. Earl), The University of Chicago, Chicago, U.S.A.

RUSSELL (Prof. L. J.), The University, Birmingham.

RYLE (G.), Christ Church, Oxford.

SCOTT (Prof. J. W.), University College, Cardiff.

SHILLINGLAW (A. T.), 9 Muirton Bank, Perth.

SHOLL (D. A.), County School, Dorking.

SIDGWICK (A.), Trewoofe Orchard, St. Buryan, Cornwall.

SMART (J. J. C.), No. 212991, c/o Imperial Bank of India, Bombay.

SMETHURST (Rev. A. F.), The Vicarage, Market Lavington, Devizes, Wilts.

SMITH (A. H.), New College, Oxford.

SMITH (Miss H. M.), Darnlee, Melrose, Roxburghshire, Scotland.

SMITH (Prof. N. Kemp), The University, Edinburgh.

SPRINKLE (Rev. H. C.), CHAPLAIN, U.S.N.R., Mocksville, N. Car., U.S.A.

STACE (Prof. W. T.), The University, Princeton, N.J., U.S.A.

STAPLEDON (W. O.), 7 Grosvenor Avenue, West Kirby, Cheshire.

STEDMAN (R. E.), University College, Dundee.

STOUT (Prof. G. F.), c/o Prof. A. K. Stout, The University, Sydney, N.S.W.

Hon. Member.

STRAWSON (P. F.), 11 Claverley Grove, Finchley, N. 3.

STRUTHERS (L. G.), 50 Bedford Square, London, W.C. 1.

STURT (H.), 55 Park Town, Oxford. *Hon. Member.*

THOMAS (L. E.), 18 Lhwynmadoc Street, Craigwen, Pontypridd, Glam.

TULLOCH (Miss D. M.), Home Lodge, Broughty Ferry, Angus.

TURNER (J. E.), 10 Middlefield Road, Liverpool, 18.

URMSON (J. O.), Magdalen College, Oxford.

VESTERLING (A. W.), 39 Barnsbury Crescent, Tolworth, Surrey.

WALKER (Rev. L. J.), S.J., Campion Hall, Oxford.
 WALLEY (J. T.), Chardleigh Green House, Chard, Somerset.
 WATERHOUSE (Prof. E. S.), The College Villa, Richmondhill, Surrey.
 WATTS (A. F.), 19 South Square, London, N.W.11.
 WEBB (Prof. C. C. J.), The Old Rectory, Pitchcott, Aylesbury, Bucks.
 WELDON (T. D.), Magdalen College, Oxford.
 WHITBY (Rev. G. S.), 5 Wilton Gardens, Glasgow, N.W.
 WHITEHEAD (Rev. L. G.), Selwyn College, Dunedin, N.Z.
 WHITEHOUSE (S. P.), The Parsonage, Dukinfield, Cheshire. *Life Member.*
 WHITELEY (C. H.), The University, Birmingham.
 WHITROW (G. J.), Christ Church, Oxford.
 WIDGERY (Prof. A. G.), Duke University, Durham, N.C., U.S.A.
 WIGHTMAN (S.), 10 Silver Birch Avenue, Fulwood, Sheffield.
 WILLIAMS (Prof. D. C.), Emerson Hall, Harvard University, Mass., U.S.A.
 WISDOM (J.), Trinity College, Cambridge.
 WISDOM (Dr. J. O.), Durdham, Chilcompton, nr. Bath.
 WODEHOUSE (Miss Helen), 8 Glanmor Crescent, Swansea.
 WOLFENDEN (J. F.), Schoolhouse, Uppingham, Rutland.
 WOLTERS (Mrs. G.), 45 Albert Road, Caversham, Reading.
 WOOLEY (A. D.), The Queen's College, Oxford.
 WRIGHT (Prof. J. N.), The University, St. Andrews.
 WROE (Rev. J. P.), D.D., Ph.D., St. John's Seminary, Womersley, Guildford.

Those who wish to join the Association should communicate with the Hon. Secretary, Mrs. KNEALE, Lady Margaret Hall, Oxford; or with the Hon. Treasurer, Mr. H. STURT, 55 Park Town, Oxford, to whom the yearly subscription of sixteen shillings should be paid. Cheques should be made payable to the Mind Association, Westminster Bank, Oxford. Members may pay a Life Composition of £16 instead of the annual subscription.

In return for their subscriptions members receive MIND gratis and post free, and (if of 3 years' standing) are entitled to buy back-numbers of both the Old and the New Series at half-price.

Members resident in U.S.A. may pay the subscription (\$4) to the Hon. Assistant-Treasurer, Prof. B. Blanchard, Swarthmore College, Swarthmore, Pennsylvania.

Members are reminded that subscriptions should be sent to the present Treasurer, Mr. Sturt, and *not* to Mr. McKie.